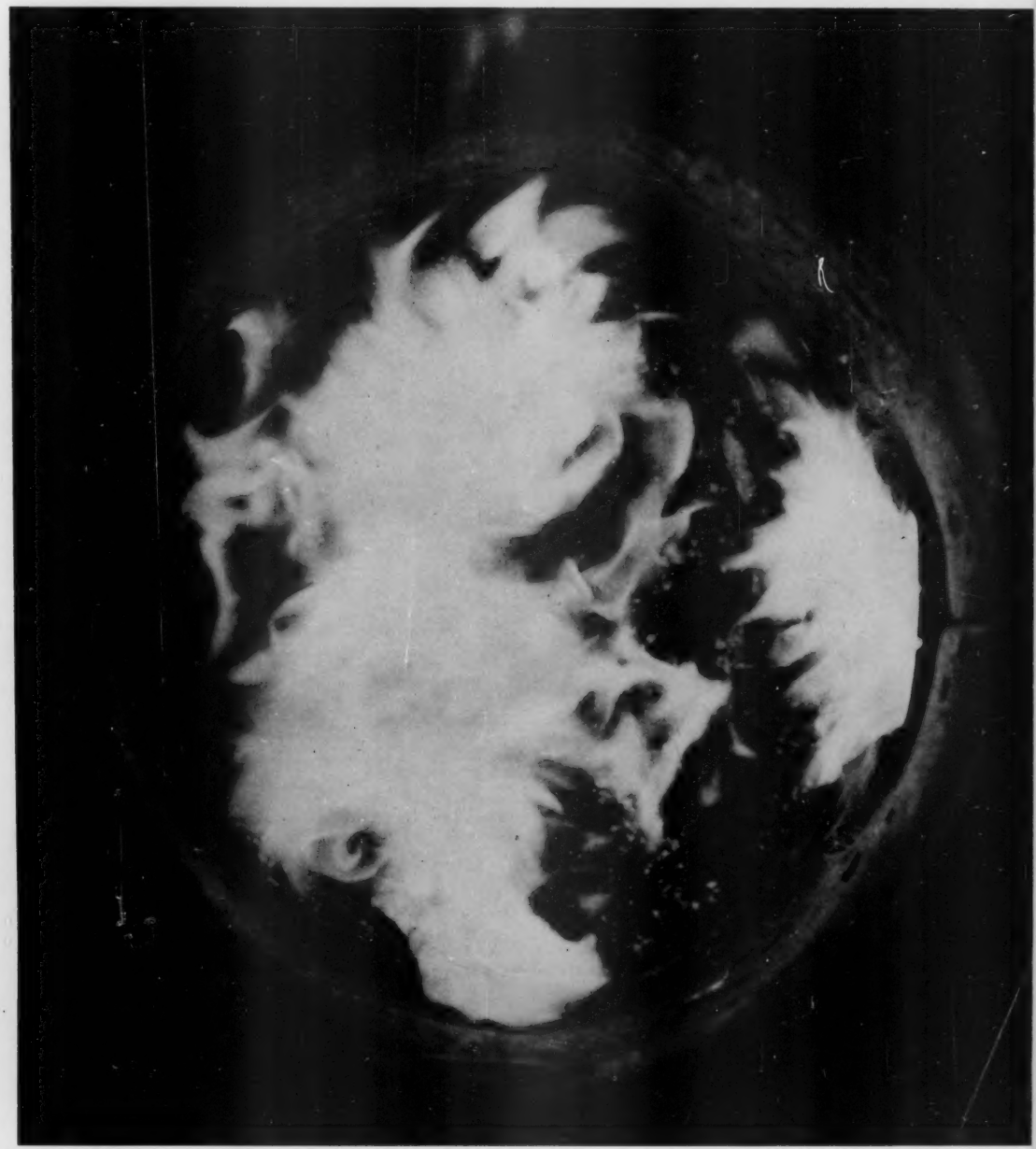
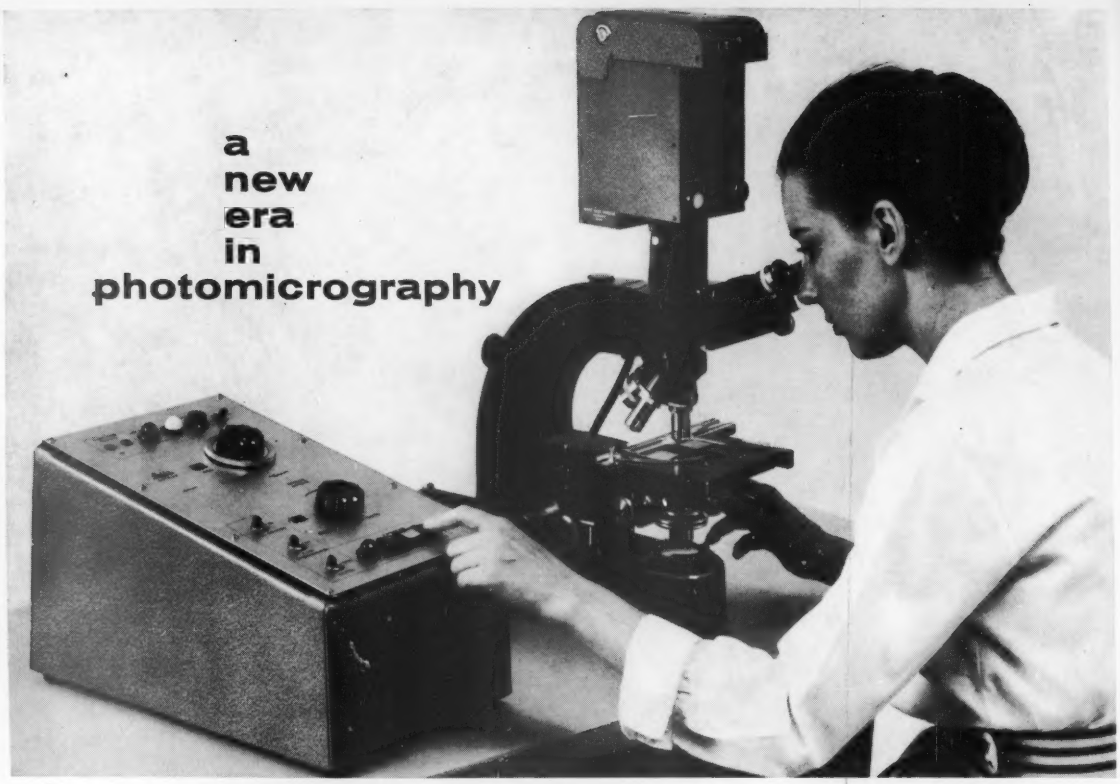
SCIENCE

1 September 1961

Vol. 134, No. 3479

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE





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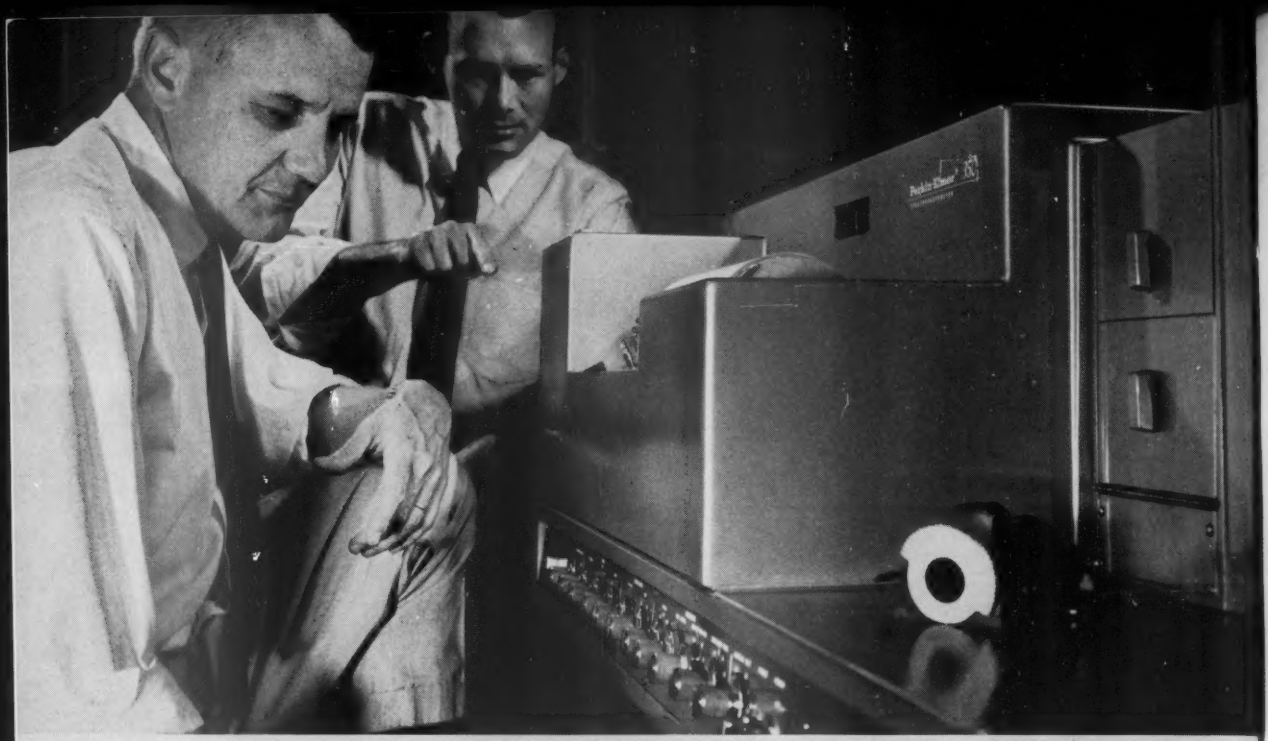

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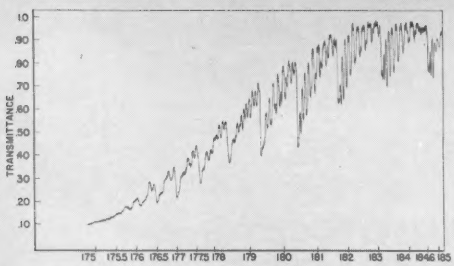
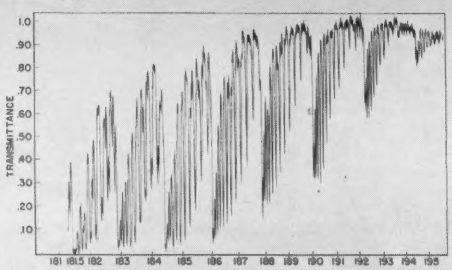


Air and nitrogen, 2 cm. cell, 14-minute scan.



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Cover

Flame pattern in an oil burner, showing the separation of the flame front during pulsation. Pulsation was detected with high-speed movies taken during a combustion chamber test. Measurements taken from the film showed that the pulsing frequency matched the frequency of high-intensity sound waves previously recorded. [Esso Research and Engineering Company]

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of the General Program of the AAAS Denver Meeting by first class mail – early in December

The *General Program* of the 128th Meeting of the AAAS in Denver, 26–31 December 1961, will be available to you, at cost, within the first week in December—whether you can attend the Meeting or not.

Program Content

1. The two-session AAAS General Sessions, "Moving Frontiers of Science," Part I—Speakers: Howard A. Meyerhoff and Arthur R. von Hippel; Harrison Brown, presiding. Part II—Speakers: Halton C. Arp and E. W. Fager; Harrison Brown, presiding.
2. The 29th John Wesley Powell Memorial Lecture. Speaker: Glenn T. Seaborg; Paul M. Gross, presiding.
3. On "AAAS Day," the four broad, interdisciplinary symposia—Physics of the Upper Atmosphere; Geochemical Evolution—The First Five Billion Years; Existing Levels of Radioactivity in Man and His Environment; and Water and Climate—arranged by AAAS Sections jointly.
4. The Special Sessions: AAAS Presidential Address and Reception; Joint Address of Sigma Xi and Phi Beta Kappa by Harrison Brown; the Tau Beta Pi Address; National Geographic Society Illustrated Lecture; and the second George Sarton Memorial Lecture.
5. The programs of all 18 AAAS Sections (specialized symposia and contributed papers).
6. The programs of the national meetings of the American Astronomical Society, American Society of Criminology, American Nature Study Society, American Society of Naturalists, American Society of Zoologists, Beta Beta Beta Biological Society, Biometric Society (WNAR), National Association of Biology Teachers, Scientific Research Society of America, Society for General Systems Research, Society of Protozoologists, Society of Systematic Zoology, and the Society of the Sigma Xi.
7. The multi-sessioned special programs of the American Association of Clinical Chemists, American Astronautical Society, American Meteorological Society, American Physiological Society, American Psychiatric Association, Association of American Geographers, Ecological Society of America, National Science Teachers Association, National Speleological Society—and still others, a total of some 70 to 80 participating organizations.
8. The sessions of the Academy Conference, the Conference on Scientific Communication, and the Conference on Scientific Manpower.
9. The sessions of the AAAS Cooperative Committee on the Teaching of Science and Mathematics, of the AAAS Committee on Science in the Promotion of Human Welfare.
10. Titles of the latest foreign and domestic scientific films to be shown in the AAAS Science Theatre.
11. Exhibitors in the 1961 Annual Exposition of Science and Industry and descriptions of their exhibits.

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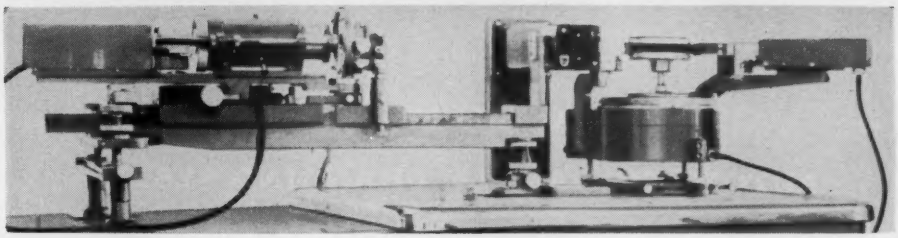
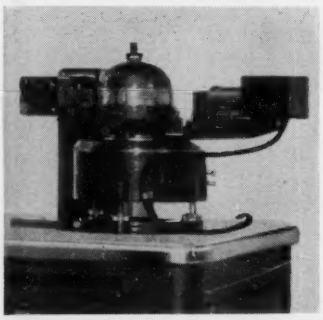
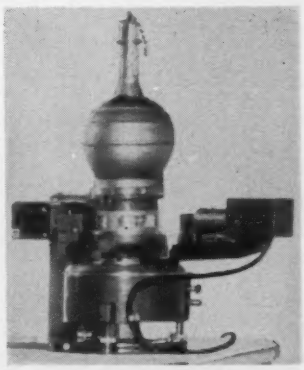


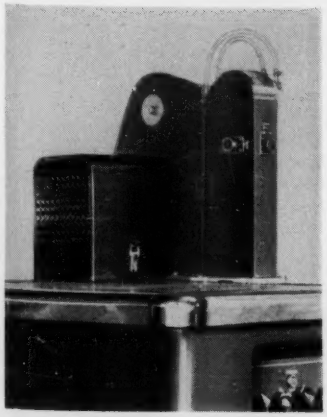
Shown above is the Rigaku Low Angle Scattering Goniometer, one of many fine instruments designed and built by Rigaku Ltd., pioneers in the x-ray diffraction equipment field since 1923. The Low Angle Scattering Goniometer has been designed for use with all standard x-ray diffraction units, to study the size, form, orientation and aggregate condition of fine individual particles of a substance. It is also used in studying the crystal periods of extra-long periodic substances, by either automatic recording or photographic techniques. It is useful in the study of organic and inorganic colloids, protein molecules, fiber micelles, resins, catalysts, clays, metals, etc.



LEFT The Rigaku High Temperature Specimen Holder is used for investigations, at high temperatures, of solubility changes as well as structural changes in the test sample. This precision instrument maintains a temperature gradient of plus or minus 5% at temperatures up to 1500°C, in vacuum or with atmospheres such as air or inert gas.

RIGHT The Rigaku Low Temperature Specimen Holder is used to investigate, at low temperatures, solubility and crystal structure changes in the specimen under survey. The temperature of the specimen is lowered to —190°C, using liquid nitrogen as the refrigerant. The investigation can be made with the specimen in an atmosphere of air, inert gas, or a vacuum.





LEFT The Rigaku Rota Unit provides the high power required for rapid analyses. Current of 100 mA at 50 KV are available from various target materials. The water cooled rotating anode is postively sealed to preclude water leaking into the vacuum. This highly reliable research tool can be used with solids, liquids or gases.

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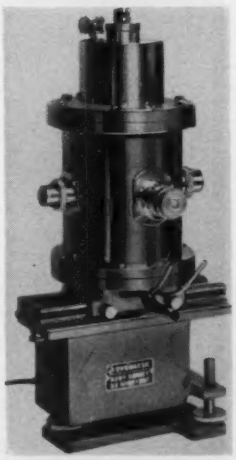

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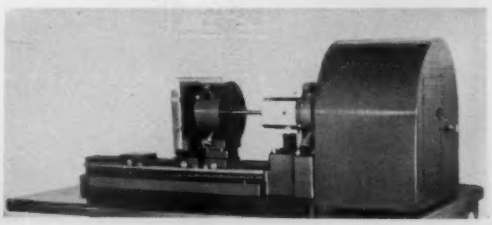

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On Being Fair though One-sided

If we piece together the different characteristics commonly ascribed to a creative scientist by the general public, and by some scientists, too, we produce the portrait of a person apparently suffering from a split personality. On the one hand, we are told that the creative scientist is distinguished by his objectivity. He is unfeeling, unmoved in his work, busy only with passive observation of phenomena. On the other hand, we are told that the creative scientist is a creature of great passion, a passion for proving his own favorite theories, or a passion for insuring, when the outcome of an experiment bears on public policy, that the outcome supports the policy he considers proper.

These opposing characteristics arise in part out of efforts at mutual correction. Each view is something of an exaggeration offered in an effort to correct the misconceptions promulgated by the opposing view. But a resolution of these apparently conflicting accounts does not consist in saying that a more accurate picture must lie somewhere between the two extremes. The extremes are there. Assuming that the scientific attitude, at least as an ideal, is not one of disharmony, a more accurate picture may be found by showing how scientists can fulfill both descriptions without contradiction.

How this may be done was nicely expressed some years ago by the philosopher and psychologist William James. In his essay "The Will To Believe," first published just before the turn of the century, James sees objectivity in science not as something impersonal and passive, but, like partisanship in behalf of a pet hypothesis, as a kind of passion, the passion not to be deceived. The scientific attitude as an ideal then emerges as the possession of two passions, as zeal in obedience to two commands, the command to gain the truth and the command to shun error.

The two commands, as James goes on to point out, are, in general, independent. Rarely is one confronted with the demand: if you do not believe this, then you must believe that. To deny that there is a pot of gold at the end of the rainbow does not commit you to the hypothesis that the pot contains silver. Occasionally, to be sure, belief in one hypothesis rules out the acceptance of another hypothesis. If you believe that the pot contains only gold, you cannot believe it contains silver. The two rules, then, are independent, and which you choose will determine the flavor of your intellectual life. You may, James continues, devote yourself to guessing the truth, paying little attention to avoiding errors. Or you may be so dedicated to avoiding error that you are prepared to let truth fend for itself.

Any attempt to sum up the scientific attitude in a few tidy phrases may justly be regarded with suspicion. After all, science is diverse both in its subject matter and in its approaches to that subject matter. But some summations are better than others, and the characterization of science as embracing simultaneously both rules strikes close to the mark. In the matter of making discoveries, unconcern is not a promising trait. But the desire to gain the truth must be balanced by an equally strong desire not to be played false.—J.T.

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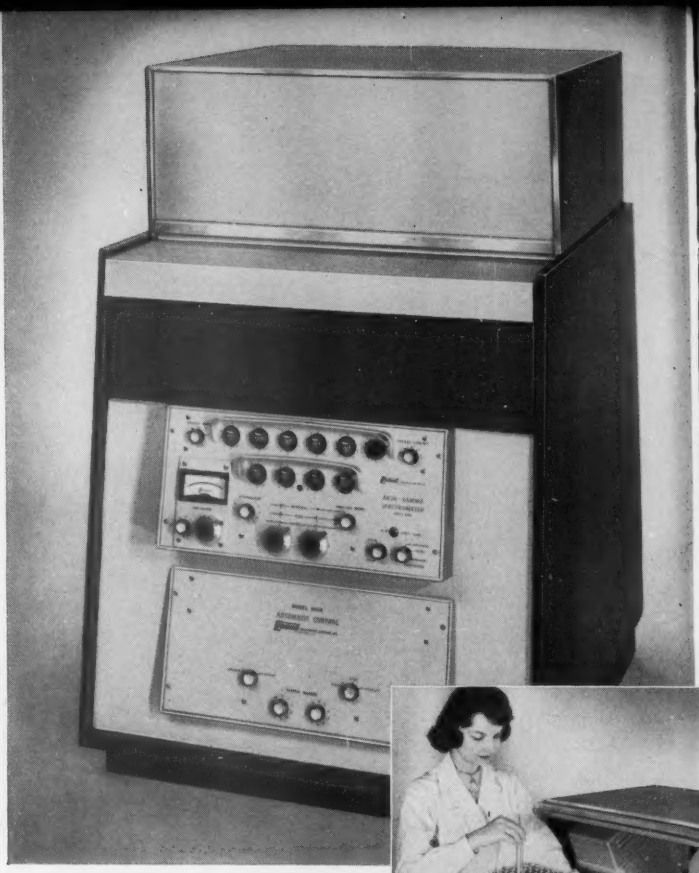

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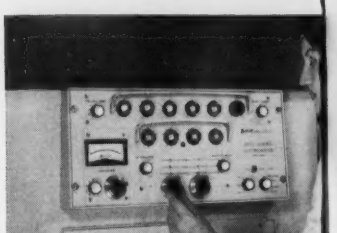

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1 September 1961, Volume 134, Number 3479 SCIENCE

CURRENT PROBLEMS IN RESEARCH

Radio Spectrum of Jupiter

Radio-frequency signals are providing important new information about the giant planet and its environs.

Alex. G. Smith

The noted scientist Vannevar Bush once said that there is no more thrilling experience for a man than to be able to say that he has learned something which no other person in the world ever knew before him. This remark was quoted by the young astronomer K. L. Franklin in describing his emotions when he and Bernard Burke accidentally discovered that Jupiter, the largest of the planets, was occasionally broadcasting powerful radio waves at a frequency of 22.2 megacycles per second (Mcy/sec).

The time was early 1955, and the two men were engaged in testing a large new receiving antenna or "radio telescope," which was to be used by the Carnegie Institution of Washington in making a radio map of the sky. At first annoyed by what seemed to be a strong, intermittent interference on many of their records, they suddenly realized that the "interference" always came precisely from the direction of Jupiter, and that it was even moving slowly among the stars at the same rate as the planet. This totally unexpected discovery marked the first recognition of radio-frequency energy from a planet (*1*, *2*).

Six years later, despite the great strides which have been made in radio astronomy, Jupiter still appears to be unique among the planets as a radio transmitter. Only the sun itself rivals the giant planet as a source of powerful outbursts in the short-wave bands of the radio spectrum. Additional interest has been lent to the problem by the recent discovery of remarkably high levels of microwave radiation, and it is probably safe to say that Jupiter is being investigated more intensively at the present time than it has been in any other period in history.

Low-Frequency Radiation

Immediately after their discovery of the 22.2-Mcy/sec radiation, Burke and Franklin re-examined records taken during the previous year with a smaller antenna at the same frequency and were able to recognize at least one instance of a strong Jovian outburst. Since a more sensitive 38.7-Mcy/sec antenna, which had been operating at the same time, failed to detect any radiation, they concluded that the sporadic radio noise is concentrated in the low-frequency region, below 38 Mcy/sec (*1*). We now know that this conclusion was correct, although in view of the highly erratic behavior of the outbursts it was based on rather scanty evidence.

Observations that my associates and I have made over a span of 5 years (*3–6*) have indicated that Jupiter has been most active at frequencies near 18 Mcy/sec, and only rarely has the radiation extended to frequencies as high as 28 Mcy/sec. In terms of the absolute energy received at the earth's surface, the Jovian bursts produce 18-Mcy/sec fluxes of as much as 10^{-19} watt per square meter per cycle per second of bandwidth, a level of energy about 200 times higher than that received from the most intense radio star. During 1960, with the help of a new field station in Chile, we were able to obtain extensive data over an unprecedented range of frequencies, and when these data were analyzed in terms of the fraction of the observing time during which radiation had been received at various frequencies, the curve shown in Fig. 1 resulted. The peaking of the activity near 18 Mcy/sec and the rapid fall-off at higher frequencies are evident. Less certain is the form of the curve below 16 Mcy/sec, because of the lack of data between 10 and 16.7 Mcy/sec and the possibility that the signals are partially absorbed by the terrestrial ionosphere at these low frequencies. During the current year we are making additional observations at 5 and at 15 Mcy/sec in an attempt to clarify this portion of the curve. These new observations should be aided by the fact that solar activity is declining rapidly from the record sunspot peak reached in early 1958, for past experience indicates that as the sun quiets down the terrestrial ionosphere becomes more transparent to radio waves of the lower frequencies (*7*).

As may be seen, the observer of the Jupiter outbursts is pretty well confined to the frequency range between a few megacycles per second and 30 Mcy/sec, by the opacity of the earth's ionosphere at the one extreme and by the absence of Jovian radiation beyond the other extreme. This part of the spectrum is one of very low frequency, by the usual standards of radio astronomy, and in many ways it is a difficult region in which to work. It lies athwart the crowded short-wave bands, so that inter-

The author is professor of physics and director of the Radio Observatory at the University of Florida, Gainesville.

ference from radio stations is a serious problem. It also lies in a region of the spectrum in which natural static from thunderstorms is severe. Moreover, since the low-frequency waves are of long wavelength, antennas of reasonable size have poor directivity—that is, they accept signals from a relatively large area of the sky. In most of our work we have been employing simple fixed arrays of from 4 to 64 dipoles, together with rotatable Yagi antennas resembling over-sized television aerials. Since receiver noise is no problem at these frequencies, commercial short-wave receivers are used to amplify the signals, which are then permanently recorded on paper by ordinary pen recorders.

The ionosphere not only acts as a screen to shut out low-frequency signals from space but it also serves as a giant mirror to reflect interference from distant terrestrial thunderstorms and radio stations into the antennas. Because the ionosphere is formed by solar radiation, it begins to fade at sunset. Between midnight and dawn, conditions are generally the most favorable for low-frequency radio astronomy, and our best observations have been made during this period. It is possible that as the sunspot cycle wanes the useful observing hours can be extended considerably.

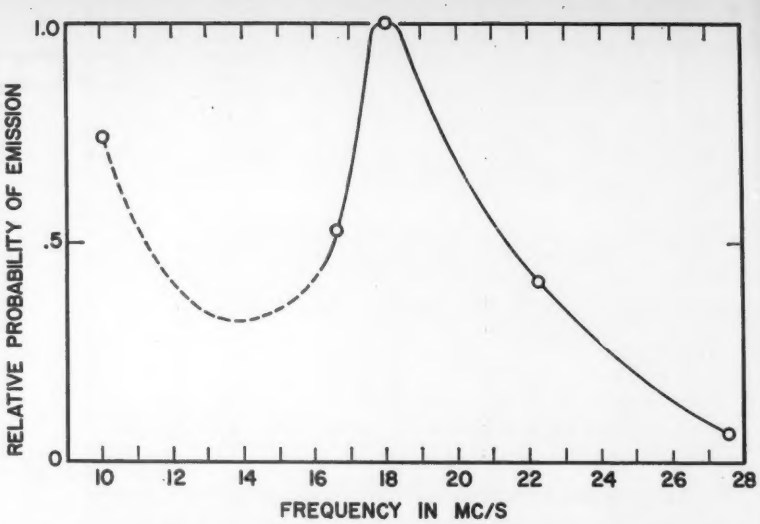

Fig. 1. A curve showing the relative probability of emission of radiation from Jupiter at various radio frequencies. This probability may be interpreted as the fraction of the total 1960 observing time during which radiation was actually received. The peak of the curve corresponds to an absolute probability of about 0.1—that is, 18-Mcy/sec signals were detected during approximately 10 percent of the observing time.

Localized Radiation Sources

As soon as he learned of the discovery of the Jovian radio outbursts, the Australian radio astronomer C. A. Shain began a search of an extensive series of records taken at 18.3 Mcy/sec in 1950 and 1951. Although these records had been made for the purpose of mapping the radio noise from the Milky Way, no less than 61 of them proved to contain disturbances which indicated by their positions that they were Jovian in origin [as in the case of Burke and Franklin's records, these signals originally had been dismissed as being due to terrestrial interference (*8*)]. Through this fortunate circumstance there immediately became available a large body of data which antedated the actual discovery by nearly 5 years. (The recovery of such pre-discovery data is not unusual in astronomy; for example, images of the planet Pluto were found on photographic plates exposed 11 years before astronomers knew of the planet's existence.)

Shain was able to show at once that there was a maximum probability of receiving radio radiation when one particular face of Jupiter was turned toward the earth as the larger planet rotated on its axis—a finding which suggested that the signals were coming from a localized source rather than from the planet as a whole. He further noted that the probability of hearing signals from this source was high only when the source was near the center of the visible disc—that is, "aimed" toward the earth. Shain speculated that this directional effect might be due to an electrified layer in the Jovian atmosphere above the source. Such an ionosphere would, as in the case of the earth, limit the escape of radio waves to a vertical cone, the angle of which would depend on the frequency of the radiation and on the electric charge density in the ionized layer.

Observations made in recent years have amply confirmed the conclusion that the radiation is from localized sources (*3–5*, *9–11*). To illustrate this point, Fig. 2 shows a histogram which summarizes the 18-Mcy/sec data of the University of Florida Radio Observatory for the year 1960. In this figure the probability of receiving radiation has been plotted as a function of the longitude of the center of the visible disc of Jupiter. It should be noted that because this visible "surface" is merely a shifting cloud mass, a purely arbitrary system of Jovian longitudes must be employed. Such systems have been established by imagining that 0 degrees of longitude was at the center of the disc at some specified instant, and that the system thereafter continued to rotate at a constant rate approximating the rotational speed of the planet itself. Since the equatorial clouds rotate appreciably faster than clouds in higher latitudes, optical astronomers have found it convenient to define separate longitude systems for keeping track of features in the two regions. "System I" rotates in $9^h50^m30^s.0$; this represents a kind of mean period for the equatorial clouds. "System II" has a period of $9^h55^m40^s.6$, keeping it approximately in step with markings in the temperate zones (*12*).

It is evident from Fig. 2 that radiation is received most frequently when longitudes near 240 degrees are turned toward the earth. There are also secondary probability peaks near 150 and 345 degrees, suggesting the presence of at least three radio sources on the planet. This same pattern of peaks tends to appear year after year, and in histograms plotted for a number of different frequencies, although in some cases the data have been too scanty for effective analysis. The figure shows clearly that the Jovian radio sources do not by any means emit continuously. Even the principal source, at 240 degrees, has a probability of emission of only a little over 0.3, indicating that in 1960 18-Mcy/sec signals were received only

about one-third of the time when this source was turned toward the earth, and the probabilities at other frequencies were even lower (Fig. 1). Evidently the activity of the sources is highly sporadic. (There also appear to be year-to-year variations in the rate of emission. In 1957 and 1960, for example, Jovian radiation was relatively frequent and intense, while in 1958 and 1959 the planet was much less active.)

The directional characteristic which Shain noted is clearly demonstrated by the relative narrowness of the peaks in Fig. 2. If the sources radiated isotropically, each source could be heard as long as it was anywhere on the visible half of the planet, and each peak would extend over a full 180 degrees of longitude. It is, then, this directional property which permits one to resolve the individual sources, for otherwise the peaks would be so broad that all of them would merge together. The same kind of argument can be used to limit somewhat the possible latitudes of the sources, about which the radio data as yet give no direct information. Since the rotational axis of Jupiter is nearly perpendicular to the plane of the ecliptic, vertical radiation cones emitted by sources at high latitudes could never strike the earth.

Unfortunately for Shain's attractive ionospheric hypothesis, our data, as well as the data of other observers (*3, 5, 9, 10*), show that the widths of the peaks decrease as the frequency of the radiation increases (see Table 1). This behavior is in direct opposition to what one would expect if radiation from a source near the surface of the planet were being limited to a "cone of escape" by an ionosphere. At the present time, therefore, the directional characteristic of the sources must be classed as one of the unsolved problems.

Table 1. Angular extent of the principal source as a function of frequency. The source width was measured at the half-amplitude height of the peak on histograms such as that of Fig. 2 (University of Florida data for 1960).

Frequency (Mcy/sec)	Peak width (deg)
10	108
16.7	54
18	43
20	36
22.2	27
27.6	20

Rotation Period of Radio Sources

In his analysis of the 1950–51 records, Shain found that the radio outbursts tended to recur with a periodicity which closely matched the System II rotation period adopted by the optical astronomers. This not only led to the concept of a localized source but also suggested that the radio source was rotating at approximately the same rate as the cloud belts in Jupiter's temperate zones (*8*). Over an interval of months, however, the source seemed to show a steady decrease in its System II longitude, and Shain decided that it was rotating slightly faster than the longitude system. We now know that this conclusion was correct, although Shain's value of 28 seconds for the difference between the periods was excessive. Later evaluations (*3, 9, 10, 13*) placed the difference closer to 10 or 11 seconds, and a recent redetermination by our group, using data which now extend over a decade, has given a figure of $9^h55^m29^s.35$ for the rotation period of the radio sources (*5*). Since J. N. Douglas and H. J. Smith recently derived, by an independent method, a value which differs from ours by only 0.02 second (*11*), the period now seems to be established with high precision.

The radio sources appear to rotate at a constant rate year after year, in contrast to the optical markings, which show highly variable periods. The erratic behavior of the optical features is to be expected, since they are entirely atmospheric phenomena of the dense clouds which perpetually shroud the planet (Fig. 3). On the other hand, the regularity of rotation of the radio sources suggests that they may be connected with the physical surface of Jupiter, either directly or through the action of a magnetic field. The period of rotation of the radio sources may thus be the previously undetermined period of rotation of the solid planet itself.

Repeated attempts have been made to correlate the radio sources with optical features. Actually, the number of quasi-permanent optical markings is very limited. The famous Great Red Spot has probably been observed since 1664, and a group of three white spots, which are identified cryptically as *FA*, *BC*, and *DE*, has persisted since 1939 (*14*). At the time that Burke and Franklin discovered the radio signals, the principal radio source almost coincided in longitude with the Red Spot, but the two soon drifted apart. Shain pointed out (*8*) that in 1951 the radio source was at the same longitude as the spot *DE*, but again the rotation rates have

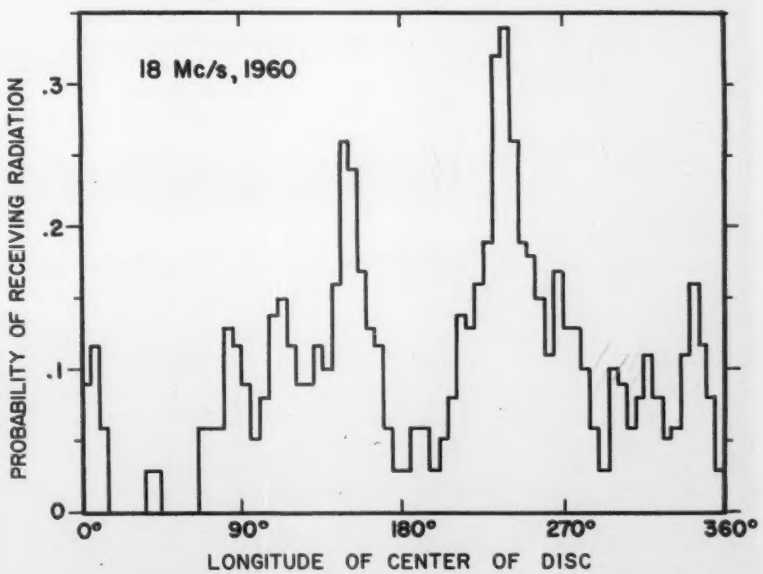

Fig. 2. A histogram showing how the probability of receiving radiation from Jupiter depends on the longitude of the center of the visible disc. For convenience in analysis, the planet has been divided into 5-degree zones of longitude. The longitude values shown here belong to System III, a new system which has been defined as rotating at the same rate as the radio sources (*3*).

proven to be different. Figure 4 shows the drift in System II coordinates of the radio sources, the Red Spot, and the three white spots during the past decade. It is evident that the periods are all quite different, and that the coincidences which have been observed from time to time have been fortuitous and temporary.

Details of the Radio Outbursts

A Jovian radio outburst may last only a few seconds, or it may continue for hours. Figure 5 shows a typical low-speed recording of a "noise storm," as the longer events have been called. High-speed recordings made during such outbursts (Fig. 6) reveal that each of the peaks in Fig. 5 is in general a complex burst consisting of a number of individual pulses. These elementary pulses commonly range from about 0.2 second to 2 seconds in duration, while the bursts themselves may last only a few seconds or as much as a minute. When a noise storm is heard in a loudspeaker it produces a distinctive rushing sound which one of my students aptly likened to waves breaking on a beach. To anticipate a question which is always asked, there is no evidence whatever to suggest that the signals are not of natural origin.

Because of the present southerly declination of Jupiter, we decided in 1959 to establish a field station at Maipú, Chile, where the planet could be observed close to the zenith, away from the concentration of terrestrial interference near the horizon (*6*). When we compared high-speed recordings taken at Maipú with those made simultaneously in Florida, some 4400 miles away, we found that the detailed correlation between the records was generally quite poor. Often when there was a strong signal in Florida there was no signal at all in Chile, while 15 or 20 seconds later the situation would be reversed (this effect may be seen clearly in Fig. 6). It appears that much of the "burstiness" of the Jupiter radiation—that is, the tendency of the signal to appear as short trains of pulses—is actually due to fading or scintillation in the terrestrial ionosphere (*15*). This conclusion is strengthened by the fact that such scintillations, with a similar fading period, have been observed for the radio stars. I am currently making a statistical comparison of the signals received at the two stations to determine to what extent the shapes of the elementary pulses themselves may be influenced by the ionosphere.

Measurements of the polarization of the Jupiter radiation—that is to say, of

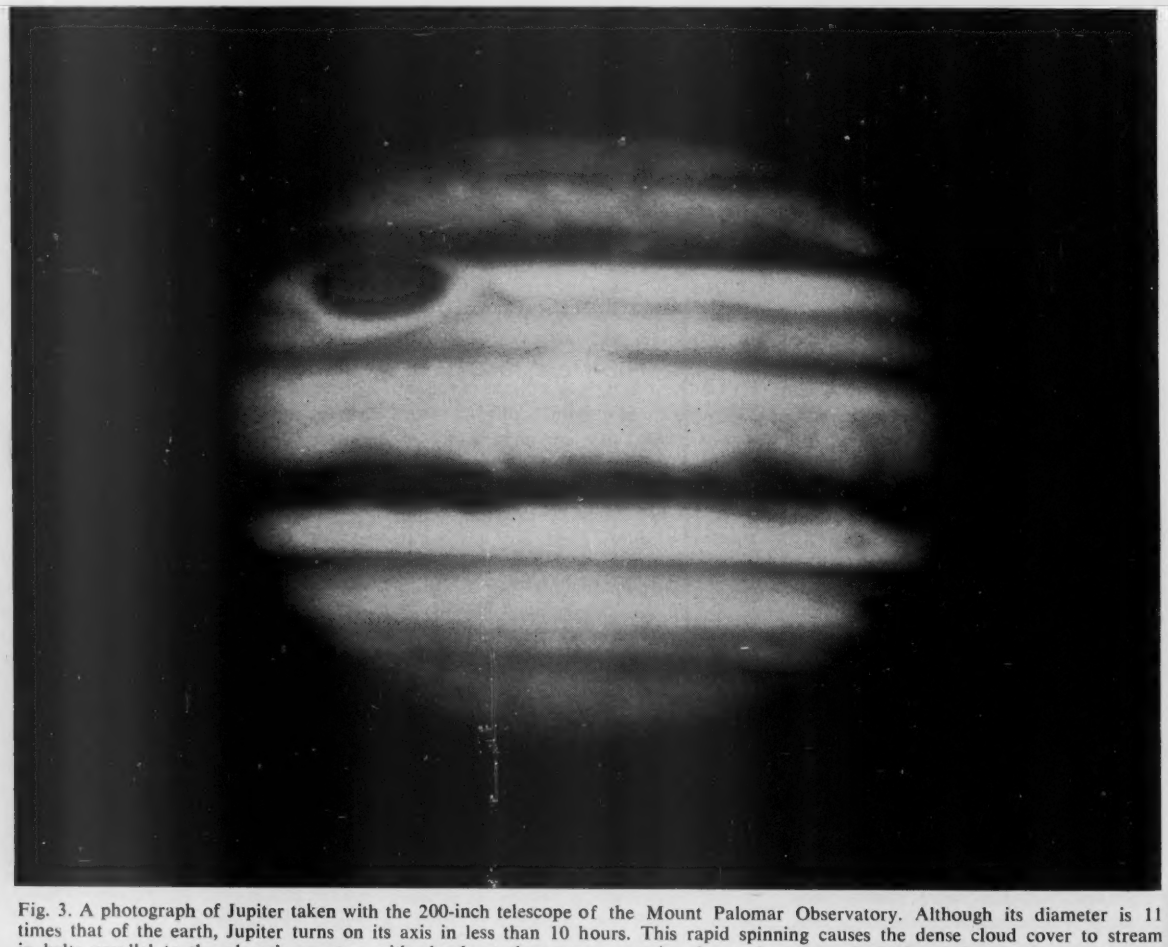

Fig. 3. A photograph of Jupiter taken with the 200-inch telescope of the Mount Palomar Observatory. Although its diameter is 11 times that of the earth, Jupiter turns on its axis in less than 10 hours. This rapid spinning causes the dense cloud cover to stream in belts parallel to the planet's equator, with clouds at the equator rotating faster than those in higher latitudes. Near the upper left edge of the disc may be seen the Great Red Spot, the longest-observed feature on the constantly changing face of the planet. [Courtesy Mount Wilson and Palomar Observatories]

the detailed manner in which the electromagnetic waves are vibrating—have been made by our group (*4, 5*), by Franklin and Burke (*9*), and by Gardner and Shain (*10*). All of the measurements have indicated that the great majority of the signals are circularly or elliptically polarized in the right-hand sense. Our results also show that there is a large pulse-to-pulse variation in the axial ratio of the polarization ellipse, although none of the observers has yet used equipment capable of determining the orientation of the ellipse.

Now, such polarization is quite likely to arise as a result of radiation passing through a magneto-ionic medium—that is, a combination of an ionosphere and a magnetic field. My associate T. D. Carr assumed a model in which (i) there was a Jovian ionosphere of the density suggested by the cone of escape of the radiation (although, as we have seen, there are difficulties with this interpretation); (ii) the radio sources were located near the latitude of greatest optical activity, that of the Red Spot; and (iii) the magnetic poles of Jupiter coincided with its axis of rotation. Using this model, Carr found that the observed polarization could be produced by a Jovian magnetic field of 7 gauss, a value about ten times that of the stronger regions of the earth's field (*4*). Although this value is admittedly only a crude estimate, it indicates that polarization measurements may ultimately reveal the latitudes of the radio sources and the presence or absence of a Jovian ionosphere, as well as providing a means of determining the strength of the magnetic field of the planet (*16*).

In 1960 we obtained simultaneous polarization records at the stations in Florida and Chile. Although the best records were marred somewhat by a partial malfunction of the Florida equipment, the data indicated that the same polarization sense was observed in both magnetic hemispheres of the earth. If this finding is verified in experiments now in progress, the terrestrial ionosphere and magnetic field will be eliminated as a possible source of the observed polarization effects.

A matter of obvious importance is the spectral bandwidth of the elementary pulses. Observations made simultaneously on channels differing in frequency by several megacycles per second seldom show much correlation between individual pulses, and it is clear that this places an upper limit on the bandwidth (*4, 9*). For several years we have been conducting experiments with two receivers connected to the same antenna. During noise storms the frequency separation of the receivers is systematically varied, while the receiver outputs are recorded on a two-channel high-speed pen recorder. Although the results are quite variable, the detailed correlation between the channels generally becomes poor when the receiver frequencies differ by a few tenths of a megacycle per second.

In 1960 we put into operation a swept-frequency receiver which continuously displays any 4-Mcy/sec segment of the spectrum on a cathode ray screen, while a motion picture camera photographs the screen several times each second. Although this equipment initially was able to record only the strongest bursts, it confirmed the belief that many of the elementary pulses had half-amplitude bandwidths of the order of several tenths of a megacycle per second, and it gave graphic pictures of the build-up and decay of the pulses (Fig. 7). The pulses often appeared to be bifurcated, and occasionally three or even more peaks at different frequencies were evident. The sensitivity of the equipment has now been increased, so that pulses of only moderate intensity are being recorded. Many of these show broader spectra, with half-widths around 1 Mcy/sec.

Noise storms commonly drift up or down the spectrum with the passage of time (*5*). Sometimes a storm will appear first on the highest-frequency channel and then show up at successively lower frequencies; or the drift may be in the opposite direction. Our 1960 data indicate that drifts toward lower frequencies were the more common. The rate of drift is highly variable, but values of a few megacycles per second per minute are often observed.

Microwave Radiation

A broad spectrum of so-called "thermal radiation" is emitted by every object which has a temperature above absolute zero. For bodies at the temperatures of typical planetary surfaces, this radiation peaks in the infrared and falls off rapidly at longer wavelengths. In the radio part of the spectrum the energy declines as the square of the wavelength, so that the best opportunity for detecting thermal radio emission from a planet lies in the microwave region. Even here, the largest antennas and most sensitive receivers must be employed. By making quantitative measurements of thermal radiation it is

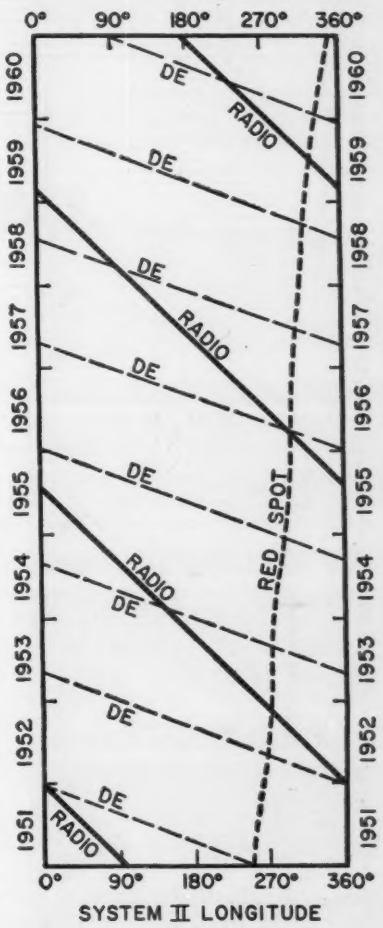

Fig. 4. A plot showing how both the radio sources and the few long-enduring optical features have drifted with respect to System II longitude coordinates during the past decade. To reduce confusion, only the position of the principal radio source is given, and the three white spots *FA*, *BC*, and *DE* are represented by the drift line of *DE* alone. The relatively slow drift of the Red Spot indicates that its period nearly matches that of System II, while the "waviness" in the drift line is due to irregularities in the Spot's motion. Although the white spots are near the latitude of the Red Spot, they rotate much more rapidly, gaining on the Red Spot by a complete lap in a little over a year. Notice that while the radio-source rotation period is only 11 seconds shorter than the rotation period of System II, the radio sources have gained nearly three laps on System II during the time they have been under observation. It is evident that the various drift lines periodically cross each other, producing temporary conjunctions of the radio sources and the visual markings.

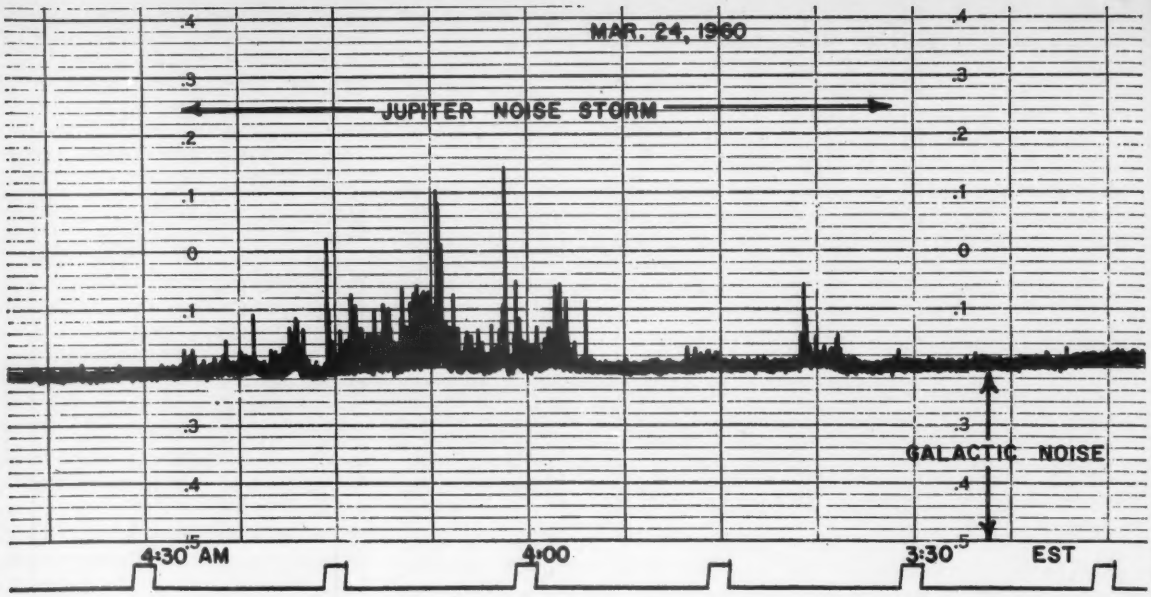

Fig. 5. A low-speed recording of a Jovian noise storm, made at the University of Florida at a frequency of 18 Mcy/sec. This storm, which lasted about an hour, was of moderate intensity and duration. The planetary bursts are always superimposed on a steady background signal emitted by our Galaxy (the Milky Way).

possible to compute the temperature of the source. Such measurements have long been made in the infrared by optical astronomers, and temperatures have been derived for the sun, the moon, and most of the planets. Early in the history of radio astronomy, radio temperatures were determined for the sun, and somewhat later, for the moon.

Thermal radio emission from a planet was first measured by Mayer, McCullough, and Sloanaker, in May of 1956. Using the 50-foot parabolic antenna of the Naval Research Laboratory at a wavelength of 3.15 centimeters, they obtained signals of sufficient strength from both Venus and Jupiter to permit estimation of the temperatures of those bodies (*17*). The value for Jupiter turned out to be 140°K, with an uncertainty of about 56°—a value quite compatible with the long-accepted value of 130°K, which had been derived from the infrared data. During the next 2 years the radio measurement was repeated by several groups, all of whom worked at wavelengths near 3 centimeters. None of the temperatures obtained seemed to differ very significantly from the original value of Mayer *et al.*, although it began to appear that the radio temperatures were always slightly higher than the values derived from optical data (*17, 18*).

The first real surprise was provided by McClain and Sloanaker, who made 60 measurements at a wavelength of 10.3 centimeters during the summer of 1958, using the Naval Research Laboratory's new 84-foot parabolic radio telescope (*19*). The individual temperature

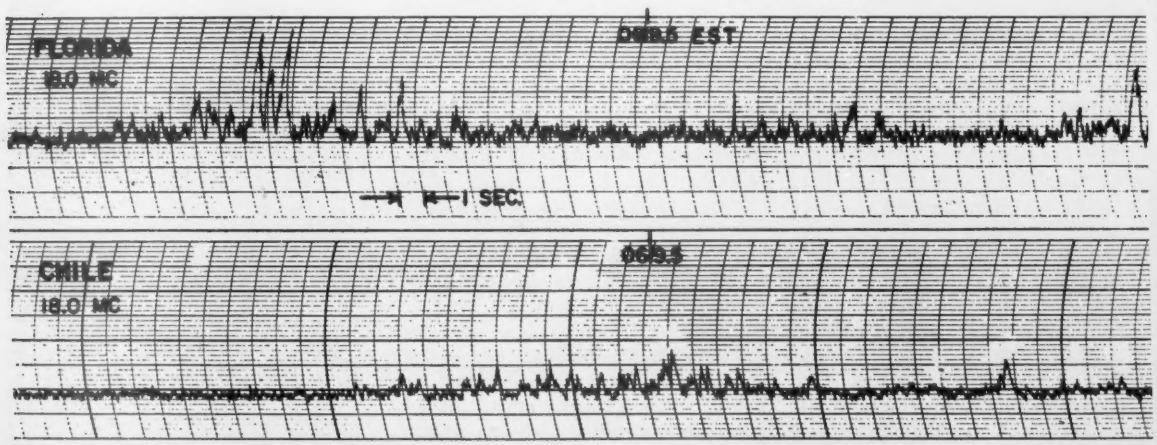

Fig. 6. High-speed recordings made simultaneously in Florida and in Chile, showing out-of-phase fading or scintillation. These high-speed recordings also resolve the elementary pulses of which the radiation is composed.

values ranged from 390° to 860°K, with a mean of 580°K. While there was some suggestion of correlation between the measured temperatures and the rotation of the planet, the experimental difficulties were such that the fluctuations could well have been due merely to statistical scatter of the data. In any event, this totally unexpected increase in the "temperature" of the planet with wavelength gave birth to immediate suspicion that a large fraction of the observed radiation might not, in fact, be thermal but might have some more complex origin, and other observers were stimulated to make measurements at still longer wavelengths.

During 1959, Roberts and Stanley found, in measurements at 31-centimeter wavelength (*20*), that the planet's "temperature" rose to 5500°K, while Drake and Hvatum made observations at 22 and 68 centimeters and found "temperatures" of 3000° and 70,000°K, respectively (*21*). The 22-centimeter data seemed to show statistically significant fluctuations, which were not, however, correlated with Jupiter's rotational period.

Figure 8 shows the dramatic way in which the computed "temperature" of Jupiter increases with wavelength if one interprets the microwave energy as being entirely of thermal origin. Of course, by 1959 it was quite obvious that most of the radiation at wavelengths above 3 centimeters must be due to some unknown phenomenon, and one more mystery was presented to the students of the giant planet.

Very recently Sloanaker and Boland reported that in late 1959 the temperature of Jupiter, in measurements at 10-centimeter wavelength, was 315°K, or about half the 1958 value. It is possible, however, that the difference was due to polarization effects resulting from the change which had occurred in the orientation of the planet with respect to the antenna (*22*).

Possible Mechanisms

There has been no dearth of speculation regarding the actual mechanism by which Jupiter emits its peculiar radio spectrum. Almost concurrently with the discovery of the decameter-wavelength radiation in 1955 there were suggestions that the outbursts might be ordinary "static" due to lightning-like discharges in the Jovian atmosphere (*8, 23*). A. J. Higgs, in fact, had suggested in 1951 the desirability of searching for such noise from Venus (*24*).

It shortly appeared that there were serious difficulties with the "lightning" theory. The narrow bandwidth and relatively long duration of the Jovian pulses were quite unlike terrestrial static. Furthermore, calculations indicated that the peak radio-frequency energy emitted by Jupiter amounted to about 10 kilowatts per cycle per second of bandwidth, an amount exceeding by several orders of magnitude the radiation due to terrestrial lightning strokes (*25*). As early as 1955, F. G. Smith discounted the lightning hypothesis and proposed that the energy for the radio outbursts might be supplied by the differential rotation of the planet—that is, by the slippage of the various atmospheric belts at different latitudes with

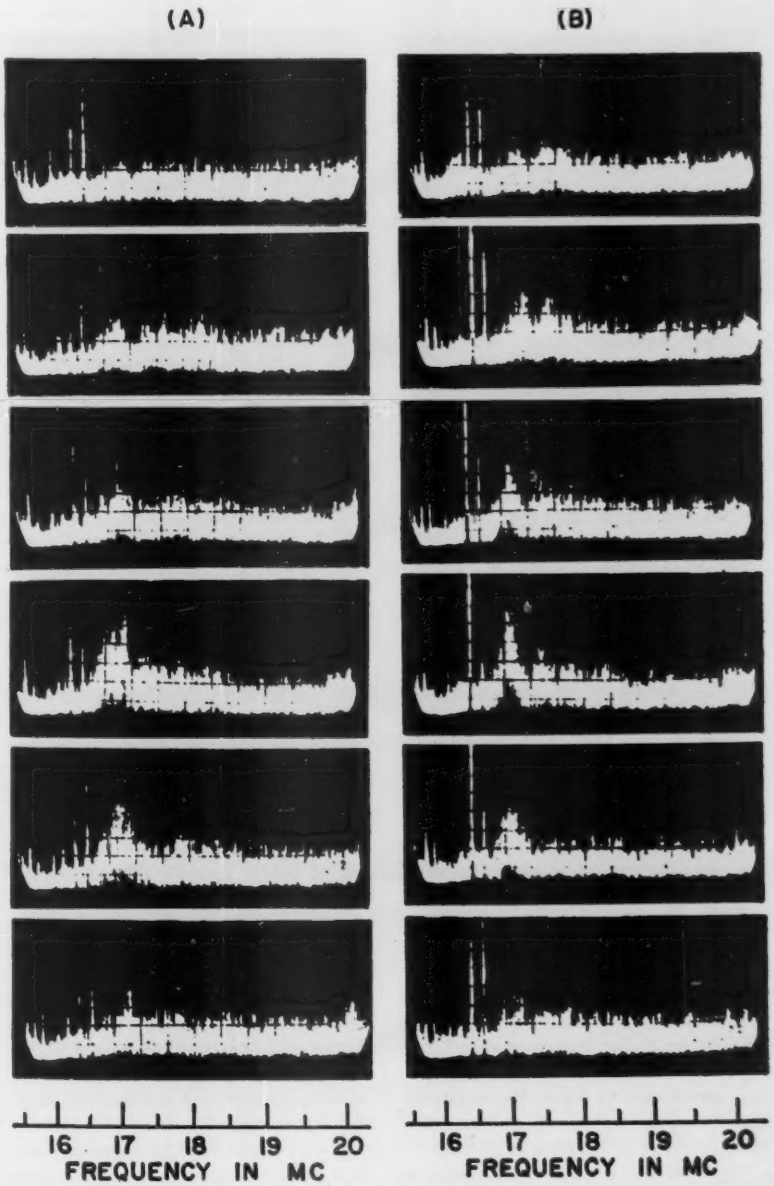

Fig. 7. Dynamic spectra of two Jupiter noise pulses near 17 Mcy/sec. Pulse *B* occurred about 6 seconds after pulse *A*. Time increases downward in each column at a rate of ½ second per frame, and the frequency scale is shown at the bottom. The narrow vertical lines at the left of the pulses are signals from radio stations. Note the splitting of pulse *B* in the first four frames.

respect to each other and with respect to the surface of the planet (*26*). In 1960 Field revived this idea, reasoning that electrical discharges could result from voltages induced by Jupiter's magnetic field slipping through its atmosphere (*27*).

In 1958 Zhelezniakov suggested that the decameter radiation is due to plasma oscillations in a Jovian ionosphere (*28*). A plasma is, of course, a highly ionized medium, and it has been demonstrated that if such a medium can be set into oscillation under favorable conditions it will emit radio waves. An attractive aspect of this theory was that such oscillations are generally believed to be responsible for similar outbursts from the sun. However, Zhelezniakov based much of his numerical calculation on the erroneous assumption that most of the Jovian radiation consists of pulses of millisecond duration, and he apparently felt that his theory could not account for the longer pulses of which the radiation appears to be composed. Gardner and Shain (*10*) and Gallet (*13*) made qualitative suggestions in 1958 that plasma oscillations might be excited by shock waves ascending from volcanic explosions on the surface of Jupiter.

Early in 1958 Kraus called attention to two occasions, in 1956 and 1957, when large solar flares were followed after an interval of several days by intense Jovian outbursts, and he proposed that the planetary noise might have been triggered by solar particles (*25*). It is well established that such solar explosions shoot out streams of charged particles, which reach the earth a day or two later and create such phenomena as the aurora borealis and various magnetic disturbances. The following year Eugene Epstein of Harvard suggested to us that if Kraus' hypothesis were correct, then when Jupiter is near opposition (that is, when the earth is between Jupiter and the sun), periods of geomagnetic activity should precede Jovian radio outbursts, since the solar particles would first strike the earth before reaching Jupiter. We were unable to look for this phenomenon in 1959, because opposition occurred during the summer, when thunderstorm activity made radio observations impossible much of the time in Florida.

By 1960 our Chilean station was in operation, and we were able to obtain radio data throughout the entire period around opposition. When these observations were compared with the geomagnetic data there was evidence that magnetic storms on the earth were indeed followed after an interval of some 5 to 12 days by Jovian radio outbursts (*29*). Warwick was making radio observations of both the sun and Jupiter in 1960, with a swept-frequency receiver, and he found an apparent correlation between Jovian noise storms and increases in the decameter radiation from the sun, with the solar activity preceding the noise storms by 1 or 2 days (*30*). The solar radio emission, like the flares, was interpreted as signaling the ejection of charged particles from the sun. In Warwick's correlation, based on an entirely different phenomenon from ours, the shorter delay time would seem to imply a much higher particle velocity, so that we have here an as yet unresolved discrepancy.

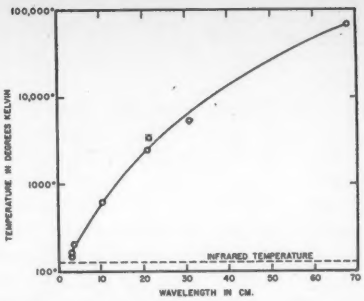

Fig. 8. A plot of the improbable way in which the "temperature" of Jupiter increases with wavelength, if the observed microwave energy is interpreted as being entirely of thermal origin. The points are mean values reported by various observers. Individual measurements fluctuate widely about these means, and in most cases it is not yet clear whether the variations are real or are merely due to the uncertainties of these very difficult observations.

If the decameter radiation from Jupiter is actually the result of bombardment of the planet by solar particles, it is not unreasonable to suppose that this bombardment might produce auroral activity, as it does here on earth. Unfortunately, we see only the daylight hemisphere of Jupiter, so that detection of such auroras poses a difficult optical problem. In 1960 we made photoelectric observations of the planet during several radio noise storms but failed to discover any significant fluctuations in its light (*5*). However, the experiment might well be repeated with more sensitive optical equipment than was then available.

Carr suggested in 1958 that the observed elliptical polarization of the low-frequency waves might be explained if the radiation were emitted directly by solar particles spiraling in the Jovian magnetic field (*31*). This concept has been developed extensively in various attempts to explain the anomalous microwave energy. Drake and Hvatum proposed that Jupiter may be surrounded by vast shells of trapped solar particles, analogous to the now-famous Van Allen belts of the earth (*21*), while Field (*27*, *32*) and Roberts and Stanley (*20*) have derived quantitative theories of such belts. According to these theories, the microwaves are either "cyclotron" or "synchrotron" radiation from electrons trapped in the planet's magnetic field.

It is well known that charged particles moving across a magnetic field are forced into circular orbits. This controlled motion, in fact, forms the basis of operation of nuclear accelerators such as the cyclotron. If the particles also have a component of velocity along the magnetic field, the circular orbits become helices centered on the lines of the field. Electrons in such orbits complete $2.8H$ million revolutions per second, where H is the magnetic field strength in gauss, and they consequently emit radio waves of the same frequency, which is known as the "cyclotron" frequency. According to the cyclotron picture of Jupiter's radiation, the observed wide band of microwave frequencies results from electrons moving in a magnetic field which varies extensively as the particles spiral back and forth from pole to pole of the planet. It is immediately obvious that the field would have to rise to over 1200 gauss to produce the 3-centimeter waves. While such a field cannot be dismissed as impossible, it is disturbingly large (the earth's external field has a maximum value of only ⅔ gauss) and it would probably produce detectable optical effects, such as splitting of spectral lines; no such effects have been observed.

If the trapped electrons possess very high energies, in the range where their relativistic increase in mass is appreciable, they will emit not only at the fundamental cyclotron frequency but at numerous higher harmonics of this frequency as well. This phenomenon is called "synchrotron" radiation, after the nuclear machine in which the effect was first observed. A synchrotron model of the Jovian emission greatly reduces the required magnetic field (the fundamental frequency can now be almost as low as one pleases), but it poses the new

problem of accounting for a density of relativistic electrons which is at present inexplicably high as compared with the density in the terrestrial Van Allen belts.

The recent discovery by Radhakrishnan and Roberts (*33*) that the microwave radiation is strongly plane-polarized parallel to Jupiter's equator is consistent with either the cyclotron or the synchrotron theory. By using two 90-foot parabolic antennas as a huge interferometer, these observers have also shown that the microwave energy apparently comes from a region having about three times the diameter of Jupiter itself, confirming the theory that there is some kind of belt or halo about the planet. Needless to say, this region may constitute a most formidable radiation hazard to approaching space vehicles.

Conclusion

One of the outstanding mysteries in the young science of radio astronomy is presented by the radio spectrum of Jupiter. Throughout most of the ordinary short-wave bands the sporadic radiation from the planet is stronger than that from any other celestial source except the sun. Recently Jupiter has been shown to emit microwave energy of such intensity that it cannot be due to simple heat radiation. Lying between these two regions of the radio spectrum is a wide gap in which no Jovian energy has been detected.

The decameter radiation is strongly correlated with the rotation of the planet, a finding that suggests the existence of localized sources. It is suspected that the microwave radiation also fluctuates, but no connection with Jupiter's rotation has yet been demonstrated. The outstanding problem at the present time is that of establishing the mechanism by which the radio energy is generated; at the moment it is not even clear whether the two observed frequency bands have a common origin. Recent observations of the polarization and angular extent of the microwave energy are consistent with theories attributing that radiation to cyclotron or synchrotron emission from electrons spiraling in a Jovian magnetic field, but these models imply either a disturbingly large field or an inexplicably high flux of energetic electrons.

It is evident that much more observational material is required, in order to establish the temporal characteristics of the microwave radiation, to study in greater detail the spectra and polarization of the decameter outbursts, and to investigate the suspected correlation with solar and geophysical phenomena. In the latter connection the noise storms may prove to be an important tool for studying the propagation of solar disturbances beyond the orbit of the earth. Moreover, the radio signals, besides shedding new light on the physical conditions of Jupiter and its environs, might conceivably be used to guide space probes toward the giant planet, making it a relatively easy target. On the other hand, the neighborhood of Jupiter will obviously be a very noisy one from the point of view of space communications.

In the 6 years which have elapsed since the discovery of the intense sporadic radiation from Jupiter, no other planet has been shown to be a source of such outbursts. For a while it was believed that Venus was a strong emitter of decameter radiation (*34*), and some weak sporadic activity from Saturn has been suspected (*4, 5, 35*), but at present Jupiter still appears to be unique as a radio source.

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Resistance by Scientists to Scientific Discovery

This source of resistance has yet to be given the scrutiny accorded religious and ideological sources.

Bernard Barber

In the study of the history and sociology of science, there has been a relative lack of attention to one of the interesting aspects of the social process of discovery—the resistance on the part of scientists themselves to scientific discovery. General and specialized histories of science and biographies and autobiographies of scientists, as well as intensive discussions of the processes by which discoveries are made and accepted, all tend to make, at the most, passing reference to this subject. In two systematic analyses of the social process of scientific discovery and invention, for example—analyses which tried to be as inclusive of empirical fact and theoretical problem as possible—there is only passing reference to such resistance in the one instance and none at all in the second (*1*). This neglect is all the more notable in view of the close scrutiny that scholars have given the subject of resistance to scientific discovery by social groups other than scientists. There has been a great deal of attention paid to resistance on the part of economic, technological, religious, and ideological elements and groups outside science itself (*1–3*). Indeed, the tendency of such elements to resist seems sometimes to be emphasized disproportionately as against the support which they also give to science. In the matter of religion, for example, are we not all a little too much aware that religion has resisted scientific discovery, not enough aware of the large support it has given to Western science? (*4, 5*).

The mere assertion that scientists themselves sometimes resist scientific discovery clashes, of course, with the sterotype of the scientist as "the open-minded man." The norm of open-mindedness is one of the strongest of the scientist's values. As Philipp Frank has recently put it, "Every influence of moral, religious, or political considerations upon the acceptance of a theory is regarded as 'illegitimate' by the so-called 'community of scientists.'" And Robert Oppenheimer emphasizes the "importance" of "the open mind," in a book by that title, as a value not only for science but for society as a whole (*6*). But values alone, and especially one value by itself, cannot be a sufficient basis for explaining human behavior. However strong a value is, however large its actual influence on behavior, it usually exerts this influence only in conjunction with a number of other cultural and social elements, which sometimes reinforce it, sometimes give it limits.

This article is an investigation of the elements within science which limit the norm and practice of "open-mindedness." My purpose is to draw a more accurate picture of the actual process of scientific discovery, to see resistance by scientists themselves as a constant phenomenon with specifiable cultural and social sources. This purpose, moreover, implies a practical consequence. For if we learn more about resistance to scientific discovery, we shall know more also about the sources of acceptance, just as we know more about health when we successfully study disease. By knowing more about both resistance and acceptance in scientific discovery, we may be able to reduce the former by a little bit and thereby increase the latter in the same measure.

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Helmholtz, Planck, and Lister

Although the resistance by scientists themselves to scientific discovery has been neglected in systematic analysis, it would be surprising indeed if it had never been noted at all. If nowhere else, we should find it in the writings of those scientists who have suffered from resistance on the part of other scientists. Helmholtz, for example, made aware of such resistance by his own experience, commiserated with Faraday on "the fact that the greatest benefactors of mankind usually do not obtain a full reward during their life-time, and that new ideas need the more time for gaining general assent the more really original they are" (*7–9*). Max Planck is another who noticed resistance in general because he had experienced it himself, in regard to some new ideas on the second law of thermodynamics which he worked out in his doctoral dissertation submitted to the University of Munich in 1879. Ironically, one of those who resisted the ideas proposed in Planck's paper, according to his account, was Helmholtz: "None of my professors at the University had any understanding for its contents," says Planck. "I found no interest, let alone approval, even among the very physicists who were closely connected with the topic. Helmholtz probably did not even read my paper at all. Kirchhoff expressly disapproved . . . I did not succeed in reaching Clausius. He did not answer my letters, and I did not find him at home when I tried to see him in person at Bonn. I carried on a correspondence with Carl Neumann, of Leipzig, but it remained totally fruitless" (*10*, p. 18). And Lister, in a graduation address to medical students, warned them all against blindness to new ideas in science, blindness such as he had encountered in advancing his theory of antisepsis.

Scientists Are Also Human

Too often, unfortunately, where resistance by scientists has been noted, it has been merely noted, merely alleged, without detailed substantiation and without attempt at explanation. Sometimes, when explanations are offered, they are notably vague and all-inclusive, thus proving too little by trying to prove too much. One such explanation is contained in the frequently repeated phrase, "After all, scientists are also

human beings," a phrase implying that scientists are more human when they err than when they are right (*11*). Other such vague explanations can be found in phrases such as "*Zeitgeist*," "human nature," "lack of progressive spirit," "fear of novelty," and "climate of opinion."

As one of these phrases, "fear of novelty," may indicate, there has also been a tendency, where some explanation of the sources of resistance is offered, to express a psychologistic bias —that is, to attribute resistance exclusively to inherent and ineradicable traits or instincts of the human personality. Thus, Wilfred Trotter, in discussing the response to scientific discovery, asserts that "the mind delights in a static environment," that "change from without . . . seems in its very essence to be repulsive and an object of fear," and that "a little self-examination tells us pretty easily how deeply rooted in the mind is the fear of the new" (*12*). And Beveridge, in *The Art of Scientific Investigation*, says, "there is in all of us a psychological tendency to resist new ideas" (*13*). A full understanding of resistance will, of course, have to include the psychological dimension—the factor of individual personality. But it must also include the cultural and social dimensions—those shared and patterned idea-systems and those patterns of social interaction that also contribute to resistance. It is these cultural and social elements that I shall discuss here, but with full awareness that psychological elements are contributory causes of resistance.

Because resistance by scientists has been largely neglected as a subject for systematic investigation, we find that there is sometimes a tendency, when such resistance is noted, to exaggerate the extent to which it occurs. Thus, Murray says that the discoverer must *always* expect to meet with opposition from his fellow scientists. And Trotter goes overboard in the same way: "the reception of new ideas tends always to be grudging or hostile. . . . Apart from the happy few whose work has already great prestige or lies in fields that are being actively expanded at the moment, discoverers of new truths always find their ideas resisted" (*12*, p. 26). Such exaggerations can be eliminated by more systematic and objective study.

Finally, in the absence of such systematic and objective study, many of those who have noted resistance have been excessively embittered and moralistic. Oliver Heaviside is reported to have exclaimed bitterly, when his important contributions to mathematical physics were ignored for 25 years, "Even men who are not Cambridge mathematicians deserve justice" (*14*). And Planck's reaction to the resistance he experienced was similar. "This experience," he said, "gave me also an opportunity to learn a new fact—a remarkable one, in my opinion: A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (*10*). Such bitterness is not tempered by objective understanding of resistance as a constant phenomenon in science, a pattern in which all scientists may sometimes and perhaps often participate, now on the side of the resisters, now on that of the resisted. Instead, such bitterness takes the moralistic view that resistance is due to "human vanities," to "little minds and ignoble minds." Such views impede the objective analysis that is required.

In his discussion of the Idols—idols of the tribe, of the cave, of the marketplace, and of the theatre—Francis Bacon long ago suggested that a variety of preconceived ideas, general and particular, affect the thinking of all men, especially in the face of innovation. Similarly, more recent sociological theory has shown that while the variety of idea-systems that make up a given culture are functionally necessary, on the whole, for man to carry on his life in society and in the natural environment, these several idea-systems may also have their dysfunctional or negative effects. Just because the established culture defines the situation for man, usually helpfully, it also, sometimes harmfully, blinds him to other ways of conceiving that situation. Cultural blinders are one of the constant sources of resistance to innovations of all kinds. And scientists, for all the methods they have invented to strip away their distorting idols, or cultural blinders, and for all the training they receive in evading the negative effects of such blinders, are still as other men, though surely in considerably lesser measure because of these methods and this special training. Scientists suffer, along with the rest of us, from the ironies that evil sometimes comes from good, that one noble vision may exclude another, and that good scientific ideas occasionally obstruct the introduction of better ones.

Substantive Concepts

Several different kinds of cultural resistance to discovery may be distinguished. We may turn first to the way in which the substantive concepts and theories held by scientists at any given time become a source of resistance to new ideas. And our illustrations begin with the very origins of modern science. In his magisterial discussion of the Copernican revolution, Kuhn (*3*) tells us not only about the nonscientific opposition to the heliocentric theory but also about the resistance from the astronomer-scientists of the time. Even after the publication of *De Revolutionibus*, the belief of most astronomers in the stability of the earth was unshaken. The idea of the earth's motion was either ignored or dismissed as absurd. Even the great astronomer-observer Brahe remained a life-long opponent of Copernicanism; he was unable to break with the traditional patterns of thought about the earth's lack of motion. And his immense prestige helped to postpone the conversion of other astronomers to the new theory. Of course, religious, philosophical, and ideological conceptions were closely interwoven with substantive scientific theories in the culture of the scientists of that time, but it seems clear that the latter as well as the former played their part in the resistance to the Copernican discoveries.

Moving to the early 19th century, we learn that the scientists of the day resisted Thomas Young's wave theory of light because they were, as Gillispie says, faithful to a corpuscular model (*15*). By the end of the century, when scientists had swung over to the wave theory, the validity of Young's earlier discovery was recognized. Substantive scientific theory was also one of the sources of resistance to Pasteur's discovery of the biological character of fermentation processes. The established theory that these processes are wholly chemical was held to by many scientists, including Liebig, for a long time (*16*). The same preconceptions were also the source of the resistance to Lister's germ theory of disease, although in this case, as in that of Pasteur, various other factors were important.

Because it illustrates a variety of sources of scientific resistance to discovery, I shall return several times to the case of Mendel's theory of genetic inheritance. For the present, I mention it only in connection with the source of resistance under discussion, substantive

scientific theories themselves. Mendelian theory, it seems clear, was resisted from the time of its announcement, in 1865, until the end of the century, because Mendel's conception of the separate inheritance of characteristics ran counter to the predominant conception of joint and total inheritance of biological characteristics (*17, 18*). It was not until botany changed its conceptions and concentrated its research on the separate inheritance of unit characteristics that Mendel's theory and Mendel himself were independently rediscovered by de Vries, a Dutchman, by Carl Correns, working in Tübingen, and by Erich Tschermak, a Viennese, all in the same year, 1900.

New conceptions about the electronic constitution of the atom were also resisted by scientists when fundamental discoveries in this field were being made at the end of the 19th century. The established scientific notion was that of the absolute physical irreducibility of the atom. When Arrhenius published his theory of electrolytic dissociation, his ideas met with resistance for a time, though eventually, thanks in part to Ostwald, the theory was accepted and Arrhenius was given the Nobel prize for it (*19*). Similarly, Lord Kelvin regarded the announcement of Röntgen's discovery of x-rays as a hoax, and as late as 1907 he was still resisting the discovery, by Ramsay and Soddy, that helium could be produced from radium, and resisting Rutherford's theory of the electronic composition of the atom, one of the fundamental discoveries of modern physics. Throughout his long and distinguished life in science Kelvin never discarded the concept that the atom is an indivisible unit (*20*).

Let us take one final illustration, from contemporary science. In a recent case history of the role of chance in scientific discovery it was reported that two able scientists, who observed, independently and by chance, the phenomenon of floppiness in rabbits' ears after the injection of the enzyme papain, both missed making a discovery because they shared the established scientific view that cartilage is a relatively inert and uninteresting type of tissue (*21*). Eventually one of the scientists did go on to make a discovery which altered the established view of cartilage, but for a long time even he had been blinded by his scientific preconceptions. This case is especially interesting because it shows how resistance occurs not only between two or more scientists but also within an individual scientist. Because of their substantive conceptions and theories, scientists sometimes miss discoveries that are literally right before their eyes.

Methodological Conceptions

The methodological conceptions scientists entertain at any given time constitute a second cultural source of resistance to scientific discovery and are as important as substantive ideas in determining response to innovations. Some scientists, for example, tend to be antitheoretical, resisting, on that methodological ground, certain discoveries. "In Baconian science," says Gillispie, "the bird-watcher comes into his own while genius, ever theorizing in far places, is suspect. And this is why Bacon would have none of Kepler or Copernicus or Gilbert or anyone who would extend a few ideas or calculations into a system of the world" (*15*). Goethe too, as Helmholtz pointed out in his discussion of Goethe's scientific researches, was antitheoretical (*22*). A more recent discussion of Gothe's scientific work also finds him antianalytical and antiabstract (*15*). Perhaps Helmholtz had been made aware of Goethe's antitheoretical bias because his own discovery of the conservation of energy had been resisted as being too theoretical, not sufficiently experimental. German physicists were probably antitheoretical in Helmholtz's day because they feared a revival of the speculations of the Hegelian "nature-philosophy" against which they had fought so long, and eventually successfully.

Viewed in another way, Goethe's antitheoretical bias took the form of a positive preference for scientific work based on intuition and the direct evidence of the senses. "We must look upon his theory of colour as a forlorn hope," says Helmholtz, "as a desperate attempt to rescue from the attacks of science the belief in the direct truth of our sensations" (*22*). Goethe felt passionately that Newton was wrong in analyzing color into its quantitative components by means of prisms and theories. Color, for him, was a qualitative essence projected onto the physical world by the innate biological character and functioning of the human being.

Later scientists also have resisted discovery because of their preference for the evidence of the senses. Otto Hahn, noted for his discoveries in radioactivity, who received the Nobel prize for his splitting of the uranium atom in 1939, reports the following case: "Emil Fischer was also one of those who found it difficult to grasp the fact that it is also possible by radioactive methods of measurement to detect, and to recognize from their chemical properties, substances in quantities quite beyond the world of the weighable; as is the case, for example, with the active deposits of radium, thorium, and actinium. At my inaugural lecture in the spring of 1907, Fischer declared that somehow he could not believe those things. For certain substances the most delicate test was afforded by the sense of smell and no more delicate test could be found than that!" (*23*).

Another methodological source of resistance is the tendency of scientists to think in terms of established models, indeed to reject propositions just because they cannot be put in the form of some model. This seems to have been a reason for resistance to discoveries in the theory of electromagnetism during the 19th century. Ampère's theory of magnetic currents, for example, was resisted by Joseph Henry and others because they did not see how it could be fitted into the Newtonian mechanical model (*24*). They refused to accept Ampère's view that the atoms of the Newtonian model had electrical properties which caused magnetic phenomena. And Lord Kelvin's resistance to Clerk Maxwell's electromagnetic theory of light was due, says Kelvin's biographer (*20*), to the fact that Kelvin found himself unable to translate into a dynamical model the abstract equations of Maxwell's theory. Kelvin himself, in the lectures he had given in Baltimore in 1884, had said, "I never satisfy myself until I can make a mechanical model of a thing. If I can make a mechanical model I can understand it. As long as I cannot make a mechanical model all the way through I cannot understand; and that is why I cannot get the electromagnetic theory" (*20*). Thus, models, while usually extremely helpful in science, can also be a source of blindness.

Scientists' positions on the usefulness of mathematics is a last methodological source of resistance to discovery. Some scientists are excessively partial to mathematics, others excessively hostile. Thus, when Faraday made his experimental discoveries on electromagnetism, Gillispie tells us, few mathematical physicists gave them any serious atten-

tion. The discoveries were regarded with indulgence or a touch of scorn as another example of the mathematical incapacity of the British, their barbarous emphasis on experiment, and their theoretical immaturity (*15*). Clerk Maxwell, however, resolved that he "would be Faraday's mathematicus"—that is, put Faraday's experimental discoveries into more mathematical, general, and theoretical a form. Initial resistance was thus overcome. Long ago Augustus De Morgan commented on the antimathematical prejudice of English astronomers of his time. In 1845, he pointed out, the Englishman Adams had, on the basis of mathematical calculations, communicated his discovery of the planet Neptune to his English colleagues. Because they distrusted mathematics, his discovery was not published, and eight months later the Frenchman Leverrier announced and published his simultaneous discovery of the planet, once again on the basis of mathematical calculations. Because the French admired mathematics, Leverrier's discovery was published first, and thus he gained a kind of priority over Adams (*25*).

Mendel was another scientist whose ideas were resisted because of the antimathematical preconceptions of the botany of his time. "It must be admitted, however," says his biographer, Iltis, "that the attention of most of the hearers [when he read his classic monograph, "Experiments in Plant-Hybridization," before the Brünn Society for the Study of Natural Science in 1865] was inclined to wander when the lecturer was engaged in rather difficult mathematical deductions; and probably not a soul among them really understood what Mendel was driving at. . . . Many of Mendel's auditors must have been repelled by the strange linking of botany with mathematics, which may have reminded some of the less expert among them of the mystical numbers of the Pythagoreans. . . ." (*18*). Note that the alleged "difficult mathematical deductions" are what we should now consider very simple statistics. And it was not just the audience in Brünn that had no interest in or knowledge of mathematics. Mendel's other biographer, Krumbiegel, tells us that even the more sophisticated group of scientists at the Vienna Zoological-Botanical Society would have given Mendel's theory as poor a reception, and for the same reasons.

In some quarters the antimathematical prejudice persisted in biology for a long time after Mendel's discovery, indeed until after he had been rediscovered. In his biography of Galton, Karl Pearson reports that he sent a paper to the Royal Society in October 1900, eventually published in November 1901, containing statistics in application to a biological problem (*26*). Before the paper was published, he says, "a resolution of the Council [of the Royal Society] was conveyed to me, requesting that in future papers mathematics should be kept apart from biological applications." As a result of this, Pearson wrote to Galton, "I want to ask your opinion about resigning my fellowship of the Royal Society." Galton advised against resigning, but he did help Pearson to found the journal *Biometrika*, so that there would be a place in which mathematics in biology would be explicitly encouraged. Galton wrote an article for the first issue of the new journal, explaining the need for this new agency of "mutual encouragement and support" for mathematics in biology and saying that "a new science cannot depend on a welcome from the followers of the older ones, and [therefore] . . . it is advisable to establish a special Journal for Biometry" (*27*). It seems strange to us now that prejudice against mathematics should have been a source of resistance to innovation in biology only 60 years ago.

Religious Ideas

Although we have heard more of the way in which religious forces outside science have hindered its progress, the religious ideas of scientists themselves constitute, after substantive and methodological conceptions, a third cultural source of resistance to scientific innovation. Such internal resistance goes back to the beginning of modern science. We have seen that the astronomer colleagues of Copernicus resisted his ideas in part because of their religious beliefs, and we know that Leibniz, for example, criticized Newton "for failing to make providential destiny part of physics" (*15*). Scientists themselves felt that science should justify God and His world. Gradually, of course, physics and religion were accommodated one to the other, certainly among scientists themselves. But all during the first half of the 19th century resistance to discovery in geology persisted among scientists for religious reasons. The difficulty, as Gillispie has put it on the basis of his classic analysis of geology during this period, "appears to be one of religion (in a crude sense) *in* science rather than one of religion *versus* scientists." The most embarrassing obstacles faced by the new sciences were cast up by the curious providential materialism of the scientists themselves (*5*). When, in the 1840's, Robert Chambers published his *Vestiges of Creation*, declaring a developmental view of the universe, the theory of development was so at variance with the religious views which all scientists accepted that "they all spoke out: Herschel, Whewell, Forbes, Owen, Prichard, Huxley, Lyell, Sedgwick, Murchison, Buckland, Agassiz, Miller, and others" (*5*, p. 133; *28*, *29*).

Religious resistance continued and was manifested against Darwin, of course, although many of the scientists who had resisted earlier versions of developmentalism accepted Darwin's evolutionary theory, Huxley being not the least among them. In England, Richard Owen offered the greatest resistance on scientific grounds, while in America and, in fact, internationally, Louis Agassiz was the leading critic of Darwinism on religious grounds (*5*, *29*, *30*).

In more recent times, biology, like physics before it, has been successfully accommodated to religious ideas, and religious convictions are no longer a source of resistance to innovation in these fields. Resistance to discoveries in the psychological and social sciences that stems from religious convictions is perhaps another story, but one that does not concern us here.

In addition to shared idea-systems, the patterns of social interaction among scientists also become sources of resistance to discovery. Here again we are dealing with elements that, on the whole, probably serve to advance science but that occasionally produce negative, or dysfunctional, effects.

Professional Standing

The first of these social sources of resistance is the relative professional standing of the discoverer. In general, higher professional standing in science is achieved by the more competent, those who have demonstrated their capacity for being creative in their own right and for judging the discoveries of others. But sometimes, when discov-

eries are made by scientists of lower standing, they are resisted by scientists of higher standing partly because of the authority the higher position provides. Huxley commented on this social source of resistance in a letter he wrote in 1852: "For instance, I know that the paper I have just sent in is very original and of some importance, and I am equally sure that if it is referred to the judgment of my 'particular' friend that it will not be published. He won't be able to say a word against it, but he will pooh-pooh it to a dead certainty. You will ask with wonderment, Why? Because for the last twenty years [. . . .] has been regarded as the great authority in these matters, and has had no one tread on his heels, until, at last, I think, he has come to look upon the Natural World as his special preserve, and 'no poachers allowed.' So I must manoeuvre a little to get my poor memoir kept out of his hands" (*8*, p. 367).

Niels Henrik Abel, early in the 19th century, made important discoveries on a classical mathematical problem, equations of the fifth degree (*31*). Not only was Abel himself unknown but there was no one of any considerable professional standing in his own country, Norway (then part of Denmark), to sponsor his work. He sent his paper to various foreign mathematicians, the great Gauss among them. But Gauss merely filed the leaflet away unread, and it was found uncut after his death, among his papers. Ohm was another whose work, in this case experimental, was ignored partly because he was of low professional standing. The researches of an obscure teacher of mathematics at the Jesuit Gymnasium in Cologne made little impression upon the more noted scientists of the German universities.

Perhaps the classical instance of low professional standing helping to create resistance to a scientist's discoveries is that of Mendel. The notion that Mendel was "obscure," in the sense that his work did not come to the attention of competent and noted professionals in his field, can no longer be accepted. First of all, the proceedings volume of the Brünn society in which his monograph was printed was exchanged with proceedings volumes of more than 120 other societies, universities, and academies at home and abroad. Copies of his monograph went to Vienna and Berlin, to London and Petersburg, to Rome and Upsala (*18*). In London, according to Bateson, the monograph was received by the Royal Society and the Linnaean Society (*32*). Moreover, we know from the extensive correspondence between them—correspondence which was later published by Mendel's rediscoverer, Correns—that Mendel sent his paper to one of the distinguished botanists of his time, Carl von Nägeli of Munich (*15, 17, 18*). Von Nägeli resisted Mendel's theories for a number of reasons: because his own substantive theories about inheritance were different and because he was unsympathetic to Mendel's use of mathematics, but also because he looked down, from his position of authority, upon the unimportant monk from Brünn. Mendel had written deferentially to von Nägeli, in letters that amounted to small monographs. In these letters, Mendel addressed von Nägeli most respectfully, as an acknowledged master of the subject in which they were both interested. But von Nägeli was the victim of his own position as a scientific pundit. Mendel seemed to him a mere amateur expressing fantastic notions, or at least notions contrary to his own. Von Nägeli's letters to Mendel seem unduly critical to present readers, more than a little supercilious. Nevertheless, the modest Mendel was delighted that the great man had even deigned to reply and sent cordial thanks for the gift of von Nägeli's monograph. On both sides, von Nägeli was defined as the great authority, Mendel as the inferior asking for consideration his position did not warrant. Ironically, Mendel took von Nägeli's advice, to change from experiments on peas to work on hawkweed, a plant not at all suitable at that time for the study of inheritance of separate characteristics. The result was that Mendel labored in a blind alley for the rest of his scientific life.

Nor was von Nägeli unique. Others, such as W. O. Focke, Hermann Hoffman, and Kerner von Marilaun, also dismissed Mendel's work because he seemed "an insignificant provincial" to them. Focke did list Mendel's monograph in his own treatise, *Die Pflanzenmischlinge*, but only for the sake of completeness. Focke paid much more attention to those botanists who had produced quantitatively large and apparently more important contributions —men such as Kölreuter, Gärtner, Wichura, and Wiegmann, of higher professional standing (*33*). Certainly, in this case, quantity of publication was inadequate as a measure of professional worth. Focke's listing of Mendel served only to bring his work, directly and indirectly, to the attention of Correns, de Vries, and von Tschermak after they had independently rediscovered the Mendelian principle of inheritance.

Mendel met with resistance from the authorities in his field after his discovery was published. But sometimes men of higher professional standing sit in judgment on lesser figures *before* publication and prevent a discovery's getting into print. This can be illustrated by an incident in the life of Lord Rayleigh. For the British Association meeting at Birmingham in 1886, Rayleigh submitted a paper under the title, "An Experiment to show that a Divided Electric Current may be greater in both Branches than in the Mains." "His name," says his son and biographer, "was either omitted or accidentally detached, and the Committee 'turned it down' as the work of one of those curious persons called paradoxers. However, when the authorship was discovered, the paper was found to have merits after all. It would seem that even in the late 19th century, and in spite of all that had been written by the apostles of free discussion, authority could prevail when argument had failed!" (*34*). So says the fourth Baron Rayleigh, and we may wonder whether his remark does not still apply, some 75 years later.

Professional Specialization

Another social source of resistance is the pattern of specialization that prevails in science at any given time. On the whole, of course, as with any social or other type of system, such specialization is efficient for internal and environmental purposes. Specialization concentrates and focuses the requisite knowledge and skill where they are needed. But occasionally the negative aspect of specialization shows itself, and innovative "outsiders" to a field of specialization are resisted by the "insiders." Thus, when Helmholtz announced his theory of the conservation of energy, it met with resistance partly because he was not a specialist in what we now think of as physics. Referring in the later years of his life to the opposition of the acknowledged experts, Helmholtz said he met with such a remark as this from some of the older men: "This has already been well known to us; what does this young medical

man imagine when he thinks it necessary to explain so minutely all this to us?" (*8*, p. 97). To be sure, on the other side, medical specialists have a long history of resisting scientific innovations from what they define as "the outside." Pasteur met with violent resistance from the medical men of his time when he advanced his germ theory. He regretted that he was not a medical specialist, for the medical men thought of him as a mere chemist poaching on their scientific preserves, not worthy of their attention. In France, even before Pasteur, Magendie had met with resistance for attempting to introduce chemistry into medicine (*35*). If medicine now listens more respectfully to nonmedical science and its discoveries, it is partly because many nonmedical scientists have themselves become experts in a variety of medical-science specialties and so are no longer "outsiders."

Societies, "Schools," and Seniority

Scientific organizations, as we may safely infer from their large number and their historical persistence, serve a variety of useful purposes for their members. And of course scientific publications are indispensable for communication in science. But occasionally, when organizations or publications are incompetently staffed and run, they may serve as another social source of resistance to innovation in science. There have been no scholarly investigations into the true history of our scientific organizations and publications, but something is known and points in the direction I have suggested. In the early 19th century, for example, even the Royal Society fell on bad days. Lyons tells us that a contemporary, Granville, "severely criticized the shortcomings of the Society" during that period (*36*). Granville gave numerous instances in which the selection or rejection of papers by the Committee of Papers was the result of bad judgment. Sometimes the paper had not been read by any Fellow who was an authority on the subject with which it dealt. In other cases, none of the members of the committee who made the judgment could have had any expert opinion in the matter. It was such an incompetent committee, for example, that resisted Waterston's new molecular theory of gases when he submitted a paper making this contribution. The referee of the Royal Society who rejected the paper wrote on it, "The paper is nothing but nonsense." As a result, Waterston's work lay in utter oblivion until rescued by Rayleigh some 45 years later (*12*, p. 26). Many present-day misjudgments of this kind probably occur, although the multiplicity of publication outlets now provides more than one chance for a significant paper ignored by the incompetent to appear in print.

The rivalries of what are called "schools" are frequently alleged to be another social source of resistance in science. Huxley, for example, is reported to have said, two years before his death, " 'Authorities,' 'disciples,' and 'schools' are the curse of science; and do more to interfere with the work of the scientific spirit than all its enemies" (*37*). Murray suggests that the supposed warfare between science and theology is equaled only by the warfare among rival schools in each of the scientific specialties. Unfortunately, just what the term *school* means is usually left unclear, and no empirical evidence of anything but the most meager and unsystematic character is ever offered by way of illustration (*38*). No doubt some harmful resistance to discovery, as well as some useful competition, comes out of the rivalry of "schools" in science, but until the concept itself is clarified, with definite indicators specified, and until research is carried out on this more adequate basis, we can only feel that "there is something there" that deserves a scholarly treatment it has not yet received.

That the older resist the younger in science is another pattern that has often been noted by scientists themselves and by those who study science as a social phenomenon. "I do not," said Lavoisier in the closing sentences of his memoir *Reflections on Phlogiston* (read before the Academy of Sciences in 1785), "expect my ideas to be adopted all at once. The human mind gets creased into a way of seeing things. Those who have envisaged nature according to a certain point of view during much of their career, rise only with difficulty to new ideas. It is the passage of time, therefore, which must confirm or destroy the opinions I have presented. Meanwhile, I observe with great satisfaction that the young people are beginning to study the science without prejudice. . . ." (*15*). Or again, Hans Zinsser remarks in his autobiography, "That academies and learned societies—commonly dominated by the older foofoos of any profession—are slow to react to new ideas is in the nature of things. For, as Bacon says, *scientia inflat*, and the dignitaries who hold high honors for past accomplishment do not usually like to see the current of progress rush too rapidly out of their reach" (*39*).

Now of course the older workers in science do not always resist the younger in their innovations, nor can it be physical aging in itself that is the source of such resistance as does occur. If we scrutinize carefully the two comments I have just quoted and examine other, similar ones with equal care, we can see that *aging* is an omnibus term which actually covers a variety of cultural and social sources of resistance. Indeed, we may put it this way, that as scientists get older they are more likely to be subject to one or another of the several cultural and social sources of resistance I have analyzed here. As a scientist gets older he is more likely to be restricted in his response to innovation by his substantive and methodological preconceptions and by his other cultural accumulations; he is more likely to have high professional standing, to have specialized interests, to be a member or official of an established organization, and to be associated with a "school." The likelihood of all these things increases with the passage of time, and so the older scientist, just by living longer, is more likely to acquire a cultural and social incubus. But this is not always so, and the older workers in science are often the most ardent champions of innovation.

After this long recital of the cultural and social sources of resistance, by scientists, to scientific discovery, I need to emphasize a point I have already made. That some resistance occurs, that it has specifiable sources in culture and social interaction, that it may be in some measure inevitable, is not proof either that there is more resistance than acceptance in science or that scientists are no more open-minded than other men. On the contrary, the powerful norm of open-mindedness in science, the objective tests by which concepts and theories often can be validated, and the social mechanisms for ensuring competition among ideas new and old—all these make up a social system in which objectivity is greater than it is in other social areas, resistance less. The development of modern science demonstrates this ever so clearly. Nevertheless, some resistance remains, and it is this we seek to understand and thus perhaps

to reduce. If "the edge of objectivity" in science, as Charles Gillispie has recently pointed out, requires us to take physical and biological nature as it is, without projecting our wishes upon it, so also we have to take man's social nature, or his behavior in society, as it is. As men in society, scientists are sometimes the agents, sometimes the objects, of resistance to their own discoveries (*40*).

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Science and the News

Grand Strategy: The Administration Has a Problem That It Would Rather Not Deal With in Public

The Administration, as noted here last week, faces an interesting and delicate problem in dealing with the relationship between elements in the Defense Department and three closely tied organizations which advocate an unrelentingly aggressive prosecution of the Cold War in terms which take on a coherent meaning only in a context of preparing for a surprise nuclear attack on Russia sometime within the current decade. The basis for this interpretation of the "Forward Strategy" put forth by the Foreign Policy Research Institute of the University of Pennsylvania was reported in this space last week. The Research Institute has been financed primarily by a tax-free educational foundation, the Richardson Foundation, whose director of research, Frank Barnett, is also director of research for the Institute for American Strategy, another educational foundation, which is devoted to influencing the public to support the overt aspects of the Forward Strategy.

Public notice has been attracted to the relationships of these organizations with the Defense Department through an article by Gene M. Lyons and Louis Morton, of Dartmouth, published in the March 1961 issue of the *Bulletin of the Atomic Scientists*, and by Senator Fulbright's memorandum on right-wing activities by the military.

"The activities of the institute," Lyons and Morton wrote, "began to expand with the series of strategy seminars it has sponsored during the past 2 years. This program started with the National Strategy Seminar, sponsored jointly by the institute and the Reserve Officers Association in the summer of 1959. It was repeated in 1960 and both acted as catalysts for regional seminars held in different parts of the country. What is particularly striking about the National Strategy Seminars is that through the authorization of the Joint Chiefs of Staff, the Institute for American Strategy in effect took over the responsibility of training reserve officers on active duty, even though the

National War College, whose facilities were used, had been giving courses on strategy to senior officers of the three services as well as civilians for the past 10 years. At the same time, while the government paid for allowance, travel, facilities, and services, the Richardson Foundation provided the funds for other expenses, including the cost of developing a curriculum for the seminars, hiring a staff, securing speakers, and purchasing books and other materials to be distributed to the students without charge. The task of developing the curriculum was turned over to the Foreign Policy Research Institute and [its director, Robert] Strausz-Hupé brought his own staff to the National War College. The interrelationship of the Institute for American Strategy and the Foreign Policy Research Institute with the National Strategy Seminar was thus intimately established."

Administration Policy

Neither the Eisenhower nor the Kennedy Administration has shared the Forward Strategists' premise that planning must be based on the assumption that the Cold War can be resolved only by a total victory of one side or the other, nor the assumption that such a total victory, even if achievable, would solve the world's problems any more effectively than the total victories of the two World Wars solved the world's problems. Lacking these assumptions, neither administration could accept the "Catonic policy" (after Cato's dictum: "Carthage must be destroyed") recommended by the Forward Strategists.

What prevented the conflict between the views from becoming apparent during the Eisenhower Administration was that the Strategy Institute program for indoctrinating reserve officers and business and education leaders, and through them the general public, is different from the Forward Strategy itself in a number of interesting ways, but most importantly in muting the implication that the Forward Strategy must culminate in a surprise nuclear attack on Russia. This is no impediment to the Forward Strategists since the culmination of the strategy would be a decision in the hands of a few key men. What is necessary is merely to indoctrinate the lower-echelon groups to support the idea that a more aggressive prosecution of the Cold War is necessary, to press the Administration in power to carry out an aggressive program, and, if this fails, to support the election of a presidential candidate who will carry it out. This and other differences between *A Forward Strategy for America* and *American Strategy for the Nuclear Age*, both published by the Research Institute within a few months of one another, make *American Strategy* more palatable to the broader audience at which it is aimed, and at the same time make more subtle the disparity between the government's policy and the institute's policy: one aimed at educating the public to understand the menace of the Cold War, a policy endorsed by Eisenhower as a proper activity of the military, and the other at educating the public to the need for a really aggressive prosecution of the Cold War, which Eisenhower rejected as likely to make an all-out war inevitable.

But the line between the two approaches is not easy to draw until the policy recommendations become explicit. *American Strategy* was faced with the problem of how to make the appeal for a more aggressive Cold War strategy convincing without offering some concrete examples of the aggressive new policies, or, on the other hand, how to offer concrete examples without offending the Administration by recommending policies it would regard as reckless. *American Strategy* solves this dilemma by recommending a number of programs which at once sound suitably aggressive for a sufficiently naive audience and at the same time sufficiently absurd so that they could not be taken seriously by anyone in a responsible government position.

Thus the concluding section of *American Strategy*, under the heading "Responses to the Challenge," includes three papers: the first is a reprint of an article by Dean Acheson, which has a number of blunt and shrewd things about the nature of the Cold War but does not recommend anything particularly in contrast with already accepted government policy; the new policies come in the two concluding papers by David Sarnoff, chairman of the board of RCA, and Frank Barnett, the research director of the Richardson Foundation and of the Strategy Institute. Both are concerned mainly with mounting a great psychological warfare campaign against the Communists. Thus the recommendations in the section, "Responses to the Challenge," consist of 11 pages for Acheson's review of a wide range of generally accepted American policies, and 33 pages for examples of *American Strategy's* new policies, which consist of a letter Sarnoff wrote to Eisenhower in 1955, and on which no action seems to have been taken, and Barnett's windup article called "What is to be done" (after Lenin's famous pamphlet outlining a proposed policy for the Bolshevik wing of the Russian radical party). Barnett's article is full of talk of "amateurs at chess and politics," "science of conflict," and "power technique," and it contains a number of curious recommendations, none of which, though, seem likely to terrify the Russians. The most ambitious of these is a recommendation for a separate cabinet department for "psychopolitical warfare" which would have "at least the status and budget of the Department of Health, Education, and Welfare," on the grounds that "if we are driven into a thermonuclear corner where we can only choose either to surrender or cremate the earth, there will be no health, education, or welfare." Barnett does not make clear what this organization would do with a budget of upwards of $4 billion a year; indeed the proposal is merely tossed out, after a two-paragraph introduction, with half a dozen related projects, after which Barnett moves on to his next proposal for "citizen experts in political warfare."

Needless to say, *Forward Strategy* contains no more than token support for such stuff, nor for any proposal like that offered in another article in *American Strategy* which suggests we might best deal with the need for foreign aid by encouraging businessmen to set up branches of their corporations in the underdeveloped countries. *Forward Strategy* does suggest, though, how the same organization could put out two such different books, united mainly by a common demand for no appeasement and a tough policy toward Russia: one is aimed at an audience which might read at least a little of a book of short articles, many of them by well-known people, on the problems of the Cold War, particularly if the book is given to them (10,000 copies have already been given away) or costs, in any case, only $1.45. The other is aimed at the more elite audience that will pay $5.95 for a book on strategy by a group of scholars whose names carry no familiar ring.

"The policy-makers should always bear in mind," says *Forward Strategy* in a statement which seems to apply to *American Strategy*, "that the general public does not care deeply about strategy, and hence has little opinion

about it." And earlier in this section on public opinion: "Central to any Forward Strategy must be the thesis that there is no substitute for victory . . . . It cannot be reiterated too strongly that the precondition of any national Forward Strategy must be the cultivation of the American people's 'will to win.' All public statements by responsible officials, all policies, all actions must be examined in the light of this axiom." In the light of this axiom it is clear why *American Strategy* devotes two chapters to debunking disarmament and has never a word to say that might suggest that minimizing the likelihood of a nuclear war might be a sensible consideration in dealing with the problem of *American Policy in the Nuclear Age*: for even to talk of such things suggests one is willing to settle for something less than total victory. This total victory approach also suggests why there is such strong support for the Forward Strategy among the better-informed admirers of Senator Goldwater, who is the only leading American political figure who is bluntly for total victory and against disarmament.

Conflict

The Eisenhower Administration, partly because of the indirect approach of *American Strategy*, never came to grips with the disparity between its policies and those of the Forward Strategists to which it was indirectly lending support, even though the approach of *American Strategy* was being used in the National Strategy Seminars for 2 years before the book was published last fall.

But the situation was bound to lead to conflicts with the new Administration. The publication of the books, the growth of the program, and the frequent appearance at the strategy seminars of speakers and materials from the far right of American politics all attracted attention. The new Administration prided itself on holding a more explicit concept of over-all American strategy than the Eisenhower Administration, and therefore would be more conscious than the previous Administration of the disparity between Institute programs and Administration programs. The new Secretary of Defense, more than any of his predecessors, was interested in knowing what was happening and in establishing control over everything that was going on in the Pentagon. All of this was bound to put an end to the comfortable relationships of the Forward Strategists with the Defense Department.

It is only possible to piece together what is happening, for the very nature of the problem requires the Administration to try to hush up the whole business, so far as this is possible. A public debate on the question is bound to have ugly effects both at home and abroad. At home the Administration would have to deal with charges that it is soft on Communism, and that it is persecuting patriots whose only offense is that they want America to win the Cold War. But to answer these charges by making the issue explicit would provide the Communists with a lovely windfall of evidence that even the American government admits there is some truth to the charge that American militarists are plotting a nuclear war.

The Administration's approach, therefore, has been to proceed as quietly as possible. As a number of reporters have found, it is difficult to get officials who know what is going on even to discuss the subject, although this evasiveness may have an adverse effect in giving the impression that the influence of the Forward Strategists is much more of a problem in the Pentagon than it really is. For instance, press officers at the Pentagon assure inquirers that a recent directive tightening control of military public information and education activities in the hands of the civilian Assistant Secretary for Public Affairs does not really indicate a change in policy at all, merely an essentially meaningless clarification of a previous directive. Nevertheless it is perfectly clear that steps are being taken: Officers who were always glad to accept invitations to speak at Cold War seminars now are, quite suddenly, too busy. The long-standing policy requiring clearance for public statements on nonmilitary matters is being enforced. The conservative National Review, which should know, reports that a Research Institute government contract has been sharply cut. Active supporters of the Forward Strategy will apparently be transferred to duties where they can do no harm, as unostentatiously as possible, one gathers, by allowing them to complete their current tour in a given post, and then seeing that they are replaced by officers with views closer to those of their Commander in Chief, or, for that matter, to those of the great majority of the elected officials of both parties.

What is doubtful is that all of this can really be carried out quietly. Thus far the debate over the Fulbright memorandum has been pretty much limited to the question of extreme right wing activities carried out under an aura of official sponsorship. This was the main topic of Fulbright's memorandum, which said nothing more about the Institutes than to suggest that their relationship with the Defense Department ought to be re-examined to see whether it did not imply official support for views at variance with the Administration. Yet this latter point is the more subtle, and hence more exploitable, issue; it can be reduced to a charge that what the Administration is really after is not extreme right wingers using their official position to promote their private views, but anyone who is patriotic enough to be truly anti-communist. From Senator Goldwater's viewpoint this is a very tempting issue: at once a righteous one, and a politically promising one.—H.M.

Note: This report, perhaps unavoidably, has given the entire movement associated in one way or another with the Forward Strategy a more monolithic character than it actually has. It should be emphasized, for one thing, that this reporter does not know just how large a body of active supporters the Forward Strategists have within the Pentagon, within the Strategy Institute, or even within Strausz-Hupé's own group at the Research Institute. The three principal authors of *Forward Strategy* are Strausz-Hupé, William R. Kintner, an army colonel who had been attached to the Research Institute on temporary duty, and Stephen T. Possony, a political scientist on the faculty of Georgetown University who has been frequently employed by the National War College. These three, as they point out in the book's preface, "are alone responsible for the conceptual framework of this book and for the specific views advanced," and even the eight associate authors of the book, who contributed to one chapter or another, do not necessarily subscribe to the over-all view of the book.

There is no question that the Forward Strategy has broader support than merely the book's three principal authors. At least some of the leaders of the Strategy Institute, and its supporters within the Pentagon, are surely aware of how well the Strategy Institute's approach to alerting the public to the menace of the Cold War fits in with the notions of how public opinion should be influenced that are presented in *Forward Strategy*, and of the contrast between this approach and that of the current and earlier Administrations'. On the other hand, it is certain that many

of the people associated with the Strategy Institute programs are not active supporters of the Forward Strategy and, indeed, this applies to at least some of those who have been most prominently associated with the Strategy Institute. It is, after all, not very difficult for an organization to get people to lend their names or a certain amount of assistance to a movement to strengthen American awareness of the dangers of the Cold War, and it does not follow that people who have become associated with the movement are active supporters of what, on closer inspection, seem to be the ultimate goals of the movement.—H.M.

Fecundity and Foreign Aid

The much-debated foreign aid bill, which was en route to congressional approval this week, occasioned a new respectability for official concern about the world's population boom and the attrition it imposes on our efforts to raise living standards in underdeveloped nations.

The bill itself contains no provisions for population control, and American aid officials point out that this country is not directly spending a cent—nor has it proposed to—on lowering the birth rate anywhere in the world.

What is notable on the status of the subject, however, is that the existence of a population problem is now openly afforded recognition, from the White House downward. This is a small change, but a significant one, and it has elevated the spirits of some old-time campaigners for population control, who were inured to official indifference. For example, Robert Cook, president of the Population Reference Bureau, a private organization devoted to collecting and distributing information on population studies, commented in an interview last week that "things have changed so greatly in the last 6 months that it's hard to believe it's the same world."

Others, not nearly so optimistic, nevertheless thought the new Administration's general willingness to come to grips with problems, combined with the subject's new-found respectability, hold out more promise for their goal than they are used to living with.

A search for the source of their optimism leads to a comparison of the responses Eisenhower and Kennedy made at presidential press conferences to similar questions on birth control and its relation to the foreign aid program.

Eisenhower (2 Dec. 1959): "I cannot imagine anything more emphatically a subject that is not a proper political or governmental activity or function or responsibility. This government has not, and will not make, as far, as long as I am here, have a positive political doctrine in its program that has to do with this problem of birth control. That's not our business."

Kennedy (19 July 1961): ". . . this is a decision which goes very much to the life of a country, and it is a personal decision and a national decision which these countries must make. The problem is not altogether an economic one. We help countries which carry out different policies in this regard and it is a judgment, in my opinion, which they should make."

Kennedy's statement is not a violent departure from his predecessor's, and both statements implicitly recognize that overpopulation happens to be a problem principally in lands where racial sensitivities are high. Aside from the impossibility of imposing population policies on these lands, this country would give the Soviets a Cold War propaganda bonus if it were to seek to regulate the world's dark-skinned birth rate. However, nations seeking help according to the criteria of the Kennedy formula have found it, though it can be argued the scale has been limited.

One of the countries which we help and which has made a decision to decelerate its population growth is India. Though none of the American funds that go to boost India's economy are slated for birth control, India plans to spend some $200 million of its own funds to lower its birth rate during its new 5-year plan. This outlay, in part, is made possible by large-scale American support to other Indian projects, which otherwise would require resources now scheduled for the birth-control program. In addition, the Ford Foundation has granted India $603,000 this year for training and research in family planning and pilot projects.

Following Kennedy's press conference response and his statements on population growth in connection with the Alliance for Progress, a number of Administration officials have spoken out—not vigorously, but along lines that were usually restricted to private conversations in the previous Administration.

For example, last week, Rowland Burnstan, assistant secretary of commerce for international affairs, spoke on the Alliance for Progress at a management program sponsored by the Columbia University Graduate School of Business. Noting that "the 'population explosion' in Latin America is one of the fundamental aspects of the development problem," he warned that while population growth can be a blessing, it can also lead to political and economic instability if employment does not keep pace. He added: "Analysis of data for recent years shows that efforts in Latin America both by local governments and under the assistance programs of the United States have not reached the minimum results required." Then he dropped the subject.

Not surprisingly, in congressional discussions over the foreign aid bill, there was no effort by supporters to impede its difficult path by emphasizing the view that an uncontrolled population boom was diluting our efforts to raise living standards in the underdeveloped lands. On several occasions, Senator Fulbright, chairman of the Foreign Relations Committee, warned Administration witnesses that they had better not be oblivious to the subject, and James Reston, the New York *Times* correspondent, who is both friendly to and influential in the Administration, wrote that "Nothing is surer than that there will be a decisive revolt against foreign aid one day if the population problem is not faced."

Both in Congress and the Administration there are vivid memories of what occurred 2 years ago when it was officially acknowledged that the problem exists and that the government should do something about it. The acknowledgment was in the so-called Draper Report, produced by the President's Committee To Study the United States Military Assistance Program, which, as a high-level body, presumably with the President's ear, attracted considerable attention. The report called for the United States, on request, to help recipient countries "in the formulation of their plans designed to deal with the problem of rapid population growth. . . ."

The Roman Catholic Church reacted sharply to this call for open government involvement in birth control, in contrast to the Church's relative inactivity in regard to efforts that in effect are subsidized by the government, such as the Indian program. In a strongly worded statement, the Catholic bishops of the United States declared that "United States Catholics believe that the promotion of artificial birth pre-

vention is a morally, humanly, psychologically, and politically disastrous approach to the population problem." Adding that American Catholics are dedicated to expanding resources and improving distribution to meet population growth, they warned: "They will not, however, support any public assistance, either at home or abroad, to promote artificial birth prevention, abortion or sterilization whether through direct aid or by means of international organizations."

President Eisenhower's press conference statement, quoted above, came a week after the bishops' statement and removed the subject from official dialog.

The growth of open discussion of the subject is most apparent in the Administration, and least apparent in Congress where, even before the lacerating school-bill fight, there was no disposition to make Congress the arena for a struggle over birth control.

A "crash program of research" in population control, as was proposed by a group that ran full-page advertisements in the New York *Times* and the *Wall Street Journal* this week, would have to go the congressional route, and signs of receptivity on Capitol Hill are not visible.

Nevertheless, there is considerable work going on in this country and elsewhere, and with varying results, it is making itself felt in some of the underdeveloped lands about which we are most concerned, although not yet in the overwhelmingly Catholic nations of Latin America, where the population is expected to triple before the turn of the century.

Among some proponents of population planning, the hope is that the Church's interest in social well-being and order, combined with new developments in family planning, will make it possible to implement programs that have been effective elsewhere.

One method of oral contraception, which employs progesterone to suppress ovulation temporarily, has led John Rock, a Catholic physician who participated in its clinical testing, to express "the confident hope that the medication will prove acceptable to my Church, since it merely gives to the human intellect the means to suppress ovulation."

There are Catholic moralists who feel that Rock's medicine is better than his theology, but experience in a number of places, including Puerto Rico, suggests that doctrine may not be an insurmountable difficulty.

Announcements

A "science community" **research center** has been established in the Philippines by the National Science Development Board, a member of the International Science Foundation. Initially the center will consist of an administration building, a science hall, and an auditorium for Filipino scientists, engineers, and supporting technical personnel. The laboratory facilities will be made available to government scientists and to any other qualified research workers who wish to take up residence in the community.

The U.S. Public Health Service is soliciting inquiries concerning participation in or initiation of **regional institutes for public health educators**, to provide information and discussion on current research findings and activities in the field. (School Health Section, Division of Community Health Practice, USPHS, Washington 25, D.C.)

Meeting Notes

The Society for **Social Responsibility in Science** will hold its annual meeting at Harvard from 8 to 10 September. (Michael Rice, 365 Harvard St., Cambridge 38, Mass.)

The first international symposium on the **science of fire-fighting** will be held during the 140th national meeting of the American Chemical Society, which opens in Chicago on 3 September. The symposium, jointly sponsored by the National Academy of Sciences and ACS, will include a survey of current fire research in this country and abroad. (ACS, Division of Fuel Chemistry, 733 3rd Ave., New York 17)

Scientists in the News

Paul M. Gross, of the department of chemistry, Duke University, and president-elect of the AAAS, has been named chairman of a special committee established to develop long-range objectives for the environmental health programs of the Public Health Service.

The second Russian medical scientist to visit the United States arrived last week for extended research work under a special U.S.–U.S.S.R. scientific exchange program, signed in 1959. **Nokolai P. Yelinov**, deputy director of the Leningrad Chemical-Pharmaceutical Institute, will spend 4 months at the Laboratory of Infectious Diseases of the National Institute of Allergy and Infectious Diseases. The first Russian exchange scientist to do research under this program was **Fedor G. Uglov**, head of the Chair of Hospital Surgery at the Pavlov Medical Institute, Leningrad. Uglov spent 2 months at Baylor University's College of Medicine last spring.

Nevin S. Scrimshaw, recently appointed director of nutrition research, Massachusetts Institute of Technology, has been awarded the Order of Rodolfo Robles by the government of Guatemala in recognition of the achievements of the Institute of Nutrition of Central America and Panama, which Scrimshaw had headed since its establishment in Guatemala City in 1949.

F. Earle Lyman, chief of the extramural programs branch, National Institute of Dental Health, has been appointed to the newly created position of assistant director of the institute. He is succeeded by **Robert C. Likens**, research chemist in the institute's Laboratory of Biochemistry.

Recent faculty appointments at Stanford University:

Calvin F. Quate, research director and vice president of Sandia Corporation, will become professor of applied physics and electrical engineering.

John D. Krumboltz, psychologist at Michigan State University, will become associate professor of education and psychology.

Frank Kral and **John T. McGrath**, faculty members of the University of Pennsylvania's School of Veterinary Medicine, have been honored by the American Veterinary Medical Association for outstanding contributions to the advancement of veterinary medicine. Kral received the association's 12th International Congress Veterinary prize; McGrath received the Gaines award and medal.

James G. Thomson, head of the pathology department of the University of Cape Town, Union of South Africa, is serving as exchange professor of pathology at the University of Miami School of Medicine. **W. A. D. Anderson**, head of the School of Medicine's department of pathology, is taking Thomson's place in Cape Town.

Book Reviews

Darwin Today

Charles Darwin. The founder of the theory of evolution and natural selection. Gerhard Wichler. Pergamon Press, New York, 1961. xvii + 228 pp.

One would have thought that the year 1959 would bring us a definitive biography of Darwin. This was not to be. There were numerous symposium volumes and collections of essays which contributed signally to our understanding of Darwin and of the evolutionary theory. We witnessed the publication of a number of important contributions to the history of evolutionary thinking, among which Eiseley's *Darwin's Century* is outstanding. Yet, all attempts at Darwin biographies fell far short of their aim. Some of them are colorless compilations, while others are vicious denigrations which concentrate their efforts on proving that Darwin had no originality and that all that was good in the *Origin of Species* had been plagiarized from earlier authors. Perhaps it is just as well that no one tried to write the definitive biography. Not only has evolutionary research made such rapid advances in recent years that many questions now appear in a very different light from that of only a few decades before, but the Darwin centennial also led to the rediscovery of the Darwin notebooks, the contents of which are more revealing than anyone had dared to hope. The sources of his information and inspiration, the chronology and maturation of his ideas, his scientific method, all this appears in a new light. No one should attempt a biography of Darwin until the critical analysis of the notebook material has been completed.

Philosophical Misconceptions

He who wants to inform himself on Darwin, in the meantime, will find an objective and concise summary of Darwin's life and of his role in the history of evolutionary thought, in Gerhard Wichler's *Charles Darwin*. This is an ambitious volume. In part 1, the history of the theory of evolution from 1600 to 1859 is presented. Wichler quite rightly does not carry the history back to the ancients, because their impact on the development of evolutionary thought has been remarkably small, some classicists and historians notwithstanding. If anything, their influence has been inhibitory. Aristotle's failure, in the discussion of final causes, to make a clear distinction between causes for the development of the individual and for the development of nature as a whole (which is the proper subject of evolution), has bedeviled students of evolution until Darwin's day and up to the present. So has the futile endeavor of the metaphysical idealists to reconcile observed change with Plato's concept of the essentially unchangeable *eidos*. The history of the emancipation of our thinking from these philosophical misconceptions has not yet been written. It awaits an author who understands both philosophy and evolutionary theory. Wichler gives us a very brief glimpse. Not only is it quite impossible to cover the whole history of evolutionary thought in the 66 pages which he devotes to it, but there are numerous aspects of this history on which a great deal more original work needs to be done. For instance, what are the sources of historical thinking, a type of thinking which is so conspicuously undeveloped among the ancient and medieval philosophers, and what are the sources of thinking in terms of populations? Have the numerous books and papers on the evolution of "form" without evolution of the "essence" been a boon or a hindrance to evolutionary thinking?

Wichler's basic thesis, confirmed again and again by modern research, is that evolutionary thinking in the century before Darwin was quite widespread but that the major publications on the subject were so speculative and deductive (as well as factually ill-informed) that they did more to discredit the subject than to help it. This Darwin knew only too well, which was the main reason why he collected facts for so many years (more than 20) after the basic formulation of his theory and why he hesitated to publish until virtually forced into it by circumstances.

Part 2 of Wichler's work is a 30-page essay on the development of Darwin's thought on the subject of evolution and, in particular, on the theory of natural selection. Part 3 (104 pages) is somewhat heterogeneous; it contains a survey of Darwin's whole literary output in biology, a description of his family (ancestors and children), his life, his chief characteristics, his relations with Wallace, Hooker, Huxley, Gray, Lyell, and other friends, as well as a bibliography of his books.

The author's encyclopedic approach has enabled him to bring the reader in contact with an enormous amount of subject matter, but this very approach in so small a volume has prevented him from achieving anything more than a cursory survey. There is little in it that could be considered novel or original. The translation (from the original German) appears to be well done, though there are a few slips (like *sea rose* for *water lily*). Much of the recent English and American literature on Buffon, Lamarck, and Darwin was apparently unknown to the author, but this is compensated for by the fact that he quotes and discusses a considerable number of European, mostly German, authors whose writings are neglected by the English-speaking world. What gives the volume its special value is that it is singularly free of the various prejudices which mar other recent Darwin biographies. It can be well recommended to anyone who wants a concise, objective introduction to Darwin's life and the history of the evolutionary theory.

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Rocks' Longevity

The World of Geology. L. Don Leet and Florence J. Leet, Eds. McGraw-Hill, New York, 1961. viii + 262 pp. Illus. Cloth, $4.25; paper, $2.75.

This small book appears to be for the "lay" reader who is intelligent and inquiring but who is not necessarily the possessor of a university education, and its short chapters are units that can be read enjoyably at intervals. This achievement results from the compilers' careful adaption of works by many

authors, most of them now active but also including Louis Agassiz, the late R. A. Daly, Percy E. Raymond, and S. J. Shand. Somewhat less than half of the book is the work of Harvard men, and most of the small but adequate photographs have been supplied from the Harvard collections.

Following the introduction—a quick review of the history and of various fields of earth science—each of the 19 chapters, with one exception, is adapted from a single work and is prefaced by a page or two of information about its subject and author. Most of the earth sciences are considered, but a specific list of subjects and authors is not repeated here: it is enough to say that each author is authoritative in his field, and that the prose, supplemented by photographs and diagrams, is clear and often dramatic.

I made no point of detecting errors, since these must be attributable to the original sources, not to the present volume, but I did note a minor inconsistency. The Leets (flatly) and Simpson, Whipple, and Colbert (in qualified terms) agreed that no rocks were known which are older than 3000 million years; yet on page 43 there is a photograph of algae from Ontario ". . . which existed 3500 million years ago. . . ."

It is to be hoped that the editors or others will compile a companion volume that will: Present opposing authors on half a dozen controversial issues; present a few new lines of such research as paleotemperature, paleomagnetic, and high-pressure studies; take its illustrative examples largely from abroad.

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On Innovation and Imitation

Productivity and Technical Change. Cambridge University, Department of Applied Economics, Monograph No. 6. W. E. G. Salter. Cambridge University Press, New York, 1960. xi + 198 pp. $4.50.

To understand the process of economic growth, it is necessary to analyze developments not only in the economy as a whole but also in its component parts.

If, as Salter has done, one takes the time and the trouble to study the records of individual industries, he will be struck by two facts. First, the rise in labor productivity—that is, the upward trend in physical output per worker or per worker-hour that characterizes a growing economy—is a general phenomenon. Second, however, industries differ greatly in the rate at which labor productivity rises.

Rise in labor productivity is widespread because technical change and capital accumulation affect production in all sectors of the economy. Technical advance of almost every kind, sooner or later and in one way or another, leads to improvements in the methods, equipment, and materials used in every industry; thus technical advance increases (or tends to increase) output per man directly, and by also increasing output per machine and per unit of materials, it increases output per man indirectly. Technical progress in the capital equipment industries lowers the cost of equipment relative to the cost of labor, induces substitution of capital for labor wherever equipment is used, and thereby strengthens the tendency for output to increase in relation to labor input. Technical progress has a like effect in the industries producing fuels and other materials and supplies. Technical progress in the transport and communication industries serves to enlarge the scale of markets; and this makes possible the finer "division of labor," domestic and international, that helps raise productivity. As for saving, it proceeds at a sufficiently high rate to cause wealth to rise more rapidly than the labor force; in this way, capital accumulation joins in lowering the cost of using capital equipment, especially long-lived equipment, relative to the cost of labor, and thus reinforces the general tendency to substitute capital for labor. The increase in population and in per capita real income brought about by technical progress and savings also widens markets and creates economies of large-scale production. Rising income, in addition, finances the investments in education that help push up labor productivity everywhere.

Labor productivity rises at disparate rates in different industries because technical change varies in its impact on individual industries. Variation among industries also occurs in the ease with which capital may be substituted for labor and in the size of the economies brought about by a given increase in volume of production. Further, rates of increase in demand that occur in response to increase in income also vary, both systematically and randomly, from one class of product to another; this, too, causes industrial variation in the economies brought about by larger output. It also causes variation in the rate of investment, which determines the speed with which technical advance can be embodied in new and better equipment, and thus in the rate of increase of labor productivity.

The above summarizes part of Salter's study of trends in British and American industries. While it is possible that I have read more into what Salter has to say than he intended, it is certain that I have omitted findings that are important and interesting. For Salter concerned himself also with the relations between changes in productivity and changes in prices and in wages and with the role these relationships play in the process of adjustment of industry and employment to technical change and capital accumulation. His results extend or—always important in scientific work—confirm findings of previous studies.

Economists may be spurred by Salter's model of economic change to quarrel and perhaps to improve. For example, Salter sees the diffusion of technical change within an industry as resulting from the replacement of old plants with new ones. Replacement occurs when the direct costs (per unit of output) of manning, supplying, and maintaining old plants come to exceed the total costs of new plants—that is, their direct costs plus depreciation and interest charges. The model implies that an industry is homogeneous in all respects except vintage of plant and correlated technical level. However, in a study of the spread of hybrid corn over the United States [summarized in an earlier issue of *Science* **132**, 275 (1960)], Zvi Griliches emphasized the heterogeneity of the corn producing areas and the problem of adapting the innovation to the varying circumstances of each area. Salter's model may be appropriate for manufacturing and Griliches' for agriculture; in any case, the general applicability of Salter's model seems doubtful.

A related assumption, to which exception might be taken, is that investment is the prime vehicle of technical change. But Salter is well aware that other factors play a role in the application of technical advance. He would acknowledge that investment is a necessary but not a sufficient condition, that the rate of innovation may not in fact

be associated in any stable manner with volume of investment, and that it is important to determine just what the relationship is.

No reasonable reader could expect Salter to cover everything important that is implied by the words "productivity and technical change." Salter does not ask why technical change occurs the way it does, or what determines the rate of saving, for example. Nor does he deal (except incidentally) with the questions of policy—regarding monopoly, capital markets, money, taxes, tariffs, patent laws, agriculture—that concern everyone who asks how economic growth may be accelerated. Salter's is a scientific work—an intelligent and workmanlike piece of scientific work—of the kind needed to put solid ground under policy to stimulate growth.

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Pro and Contra Darlington

The Sounds of Language. An inquiry into the role of genetic factors in the development of sound systems. L. F. Brosnahan. Heffner, Cambridge, England, 1961. 250 pp. 25s.

Brosnahan writes well. He has a wide knowledge of linguistic, psychological, and genetic facts and the gift of clear presentation. He is fair in his presentation of other theories and modest in the claims he makes for his own. Nevertheless, I remain unconvinced of any solidity in his fundamental thesis: that inborn factors have an appreciable role in predisposing populations toward developing given types of sounds. The idea is that of the geneticist, C. D. Darlington; Brosnahan attempts to support it as a linguist.

As the author states it, the problem is "why any community of speakers should select, and indeed should be continually selecting certain articulations in preference to others" (page 7). His answer is that over long periods of time there is a tendency to move toward the sounds which are easier to produce and that these are different for each human group, depending on hereditary physiology. The matter seems to be exaggerated. The structure of the mouth and throat and the capacity to hear sounds are sufficiently developed in all humans, with rare individual and no racial exceptions, to handle all sounds used in all the languages of the world. The minor differences can hardly explain the phonetic changes which have occurred in languages. For example, what could have happened to the mouths of the forerunners of the historic Greeks to cause them to change *s* to *h*? Whatever caused this, why did it subsequently permit the Attic Greeks to bring into use new instances of *s* as a replacement for *t* before the vowel *i*? And what did the ancient pre-Greeks have in common with other human groups in scattered parts of the world, which at one time or another made the same transformation of the sibilant?

To carry conviction for this thesis, Brosnahan would have to show sound changes in relation to specific physiological characteristics of the speaking organs, but he deals rather with blood factors. Thus, he presents an apparent correlation between the geographic distribution of the O-factor in the blood and the development of dental fricatives (*th*-sounds) in Europe. Since the blood does not directly participate in the production of sounds, one would have to find some indirect link between the two facts, and this link need not be physiological as such. The development was certainly related to the movement and the influence of Germanic peoples and languages and to the effect upon these of contact with Slavic and other groups. Thus phonetics and blood show a correlation only because both reflect the distribution, movement, and mixing of historic peoples and not for any causal relation between genes and speech sounds.

A few considerations can be mentioned to support the explanation which I have given here and which is opposed to that of Darlington and Brosnahan. First of all, it should be emphasized that the correlation claimed by Darlington and Brosnahan is positive but not closely so. Furthermore, there are evidently other linguistic features with a more or less similar correlation to O-blood in Europe, for example the use of the definite and indefinite articles in Germanic and neighboring languages and the absence of these articles in Slavic and other Eastern languages. Obviously differences in the patterns of word combination cannot be explained by genes, and especially not by the same genes as those supposed to account for phonetic differences. And finally, the changes discussed by Brosnahan are found in other parts of the world, where there is no connection with O-blood.

In only one place does Brosnahan seem to deal with phonetic changes that may be physiologically induced, and that is when he speaks of Chatterji's observation of a tendency toward the fronting of sounds during recent millennia. Conceivably this is related to change tendencies which occurred during the skull's development from long-headedness to round-headedness, changes which were accompanied by reduction of the length of the palate, thereby giving less contrast to the position of back and front consonants; this could favor the elimination of certain phonetic contrasts, which would then need to be replaced by new ones. Yet, even here, the evidence is far from unmistakable. Perhaps the capacity to distinguish sounds has advanced along with changes in the cranium. At any rate there are round-headed populations whose language differentiates more front-back sound types than other, long-headed ones. Any firm conclusion on an interrelationship will have to be based on much careful study.

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Structure Analysis

X-Ray Analysis of Organic Structures. S. C. Nyburg, Academic Press, New York, 1961. xii + 434 pp. Illus. $13.

Organic chemists and biochemists who wish to deepen their understanding of the techniques of x-ray structure analysis and the results of its application to organic systems will find here a book tuned to their needs. The author's aim is to provide a foundation, "with the minimum of formal mathematics . . .," on the basis of which the reader will be able "to assess the reliability of the published results [and] appreciate fully the powers and limitations of the method."

The book is divided into two main parts. In the first, comprising a little over one-third of the book, the x-ray diffraction method of structural analysis is developed. The discussion ranges from experimental techniques (chapter 1) through crystal and molecular symmetry (chapters 2 and 3) to Fourier analysis (chapter 5) and the problems of accuracy of structure determinations

(chapter 6). The coverage is comprehensive and the treatment is in general clear. Although the discussion is quite terse in several instances, it will repay the efforts of the initiate.

The second part of the book is devoted to a discussion of crystalline organic structures, ranging from compounds of low molecular weight (chapter 7, 661 references) to crystalline and fibrous macromolecular substances (chapters 7 and 8). The survey is intended to be comprehensive up to the end of June 1958, and in some cases to June 1959. Many useful figures which represent the stereochemistry and dimensions of organic molecules are included. A section summarizes the information on bond lengths in organic molecules. In some cases the discussion of structures is somewhat superficial, but this is perhaps understandable in view of the large amount of material covered.

This book arrives at an opportune time. The technique of x-ray structure analysis of large and complex organic molecules is beginning to emerge as one of the more important methods of unraveling their architecture. For those in this field who are concerned with structural and related problems, this book should provide a good introduction to the x-ray diffraction method and a useful reference to the work which has been done.

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Practical Aid

Scientific Russian Guide. Handbook for students and professionals interested in scientific Russian. Mary A. Emery and Serge A. Emery. McGraw-Hill, New York, 1961. 191 pp. $4.50.

This manual offers elementary science readings for students who have completed approximately a year of Russian language at the college level. There are 40 readings of about 200 to 300 words each: 15 biographies of famous Russian scientists and inventors, 12 selections on various topics in mathematics and physics, 9 abstracts from Soviet journals, and 4 selections that contain some interesting predictions. All but the last four readings are accompanied by a few questions in Russian and by an extensive on-the-spot word list with idiomatic, and generally good, translations.

Appendix materials, which will be useful to some, include expressions often encountered in scientific texts, translations of isolated phrases and clauses lifted from contemporary technical texts, a list of chemical elements, and the equivalents of weights and measures. The end vocabulary is adequate for most of the book, though the authors have not succeeded (despite their claim) in including "all the words used in this textbook." This is especially true of the last four readings.

In general, the readings are related to applied physics, with some attention paid to chemistry and mathematics. Aside from material in the biographies, little, if any, attention is given to biology (and medicine), astronomy, or geology.

All in all, this is an excellent, practical textbook which students at the mentioned level should find helpful.

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Reprints

Animal Ecology. W. H. Dowdeswell. Harper, New York, 1961. 223 pp. Illus. $1.50.

Cell and Psyche. The biology of purpose. Edmund W. Sinnott. Harper, New York, 1961. 119 pp. $0.95.

Darwin's Finches. An essay on the general biological theory of evolution. David Lack. Harper, New York, 1961. 214 pp. Illus. + plates. $1.40.

The Evolution of Human Nature. C. Judson Herrick. Harper, New York, 1961. 510 pp. Illus. $2.45.

A First Course in Mathematical Statistics. C. E. Weatherburn. Cambridge Univ. Press, London, 1961. 292 pp. Illus. $2.75.

A Guide to Earth History. Richard Carrington. New American Library, New York, 1961. 284 pp. Plates. $0.75.

The History of Calculus and Its Conceptual Development. Carl B. Boyer. Dover, New York, 1949. 346 pp. Illus. $2.

How to Know the American Marine Shells. H. Tucker Abbott. New American Library, New York, 1961. 222 pp. Plates. $0.75.

Life of the Past. An introduction to paleontology. George Gaylord Simpson. Yale Univ. Press, New Haven, Conn., 1961. 210 pp. Illus. $1.45.

The Molds and Man. An introduction to the fungi. Clyde M. Christensen. Univ. of Minnesota Press, Minneapolis, 1961. 246 pp. Illus. + plates. $1.75.

The Methods of Plane Projective Geometry Based on the Use of General Homogeneous Coordinates. E. A. Maxwell. Cambridge Univ. Press, London, 1961. 249 pp. $1.95.

New Lives for Old. Margaret Mead. New American Library, New York, 1961. 475 pp. Plates. $0.75.

Science and Music. Sir James Jeans. Cambridge Univ. Press, London, 1961. 258 pp. Illus. + plates. $1.95.

A Short Account of the History of Mathematics. W. W. Rouse Ball. Dover, New York, 1960. 546 pp. Illus. $2.

The Social Insects. O. W. Richards. Harper, New York, 1961. 232 pp. Illus. $1.50.

Unresting Cells. R. W. Gerard. Harper, New York, 1961. 447 pp. Illus. $2.25.

New Books

Biological and Medical Sciences

Fluorosis. The health aspects of fluorine compounds. Edward J. Largent. Ohio State Univ. Press, Columbus, 1961. 160 pp. Illus. $3.50.

General Biology. William T. Taylor and Richard J. Weber. Van Nostrand, Princeton, N.J., 1961. 955 pp. Illus.

Genetic Aspects of Dairy Cattle Breeding. Ivar Johansson. Univ. of Illinois Press, Urbana, 1961. 271 pp. Illus. $7.50.

The Human Cerebellum. An atlas of gross topography in serial sections. Jay B. Angevine, Jr., Elliott L. Mancall, and Paul I. Yakovlev. Little, Brown, Boston, Mass., 1961. 147 pp. Illus. $15.

Human Psychological Development. Elizabeth Lee Vincent and Phyllis C. Martin. Ronald, New York, 1961. 527 pp. Illus. $6.50.

Illustrating Medicine and Surgery. Margaret C. McLarty. Williams and Wilkins, Baltimore, Md., 1960. 167 pp. Illus. $8.50.

Immunopathologie in Klinik und Forschung. Und das Problem der Autoantikörper. P. Meischer and K. O. Vorlaender, Eds. Thieme, Stuttgart, Germany, 1961. 710 pp. Illus. $22.50.

Integrated Principles of Zoology. Cleveland P. Hickman. Mosby, St. Louis, Mo., 1961. 972 pp. Illus. $7.75.

Lichen Handbook. A guide to the lichens of eastern North America. Mason E. Hale, Jr. Smithsonian Institution, Washington, D.C., 1961. 188 pp. Illus.

Microbiology. Phillip L. Carpenter. Saunders, Philadelphia, Pa., 1961. 448 pp. Illus.

Milk: The Mammary Gland and its Secretion. vols. 1 and 2. S. K. Kon and A. T. Cowie, Eds. Academic Press, New York, 1961. Illus. vol. 1, 528 pp., $14. vol. 2, 432 pp., $12.

Monograph of Monochaetia and Pestalotia. Emil Frederick Guba. Harvard Univ. Press, Cambridge, Mass., 1961. 349 pp. Illus. $10.

Physiologie Vegetale. R. Bastin. Université Catholique de Louvain, Louvain, Belgium, 1961. 324 pp.

Problems of Infection, Immunity and Allergy in Acute Radiation Diseases. N. N. Klemparskaya, O. G. Alekseyeva, R. V. Petrov, and V. F. Sosova. Translated from the Russian by Lydia Venters. R. Clarke, Ed. Pergamon, New York, 1961. 173 pp. Illus. $7.50.

Progress in Medical Genetics. vol. 1. Arthur G. Steinberg, Ed. Grune and Stratton, New York, 1961. 349 pp. Illus.

The Science of Genetics. Charlotte Auerbach. Harper, New York, 1961. 283 pp. Illus. $5.95.

Studies on Quantitative Radiation Biology. K. G. Zimmer. Oliver and Boyd, London, England, 1961. 121 pp. 15s.

Reports

Effects of 3-Methylcholanthrene and Phenobarbital on Amino Acid Incorporation into Protein

Abstract. Treatment of rats with methylcholanthrene and phenobarbital, two compounds known to enhance microsomal enzyme activity, stimulates the incorporation of free or soluble ribonucleic acid bound amino acid into proteins of cell-free liver preparations. Mitochondria, microsomes, and cell sap all contribute to the methylcholanthrene effect on the incorporation of free amino acid.

A number of compounds have recently aroused considerable interest because of their profound influence on several liver microsomal enzyme systems (*1–8*). Richardson *et al.* (*1*) observed that 3-methylcholanthrene (MC), when administered at low levels in the diet, markedly inhibited tumor formation in the livers of rats fed aminoazo dyes. Miller *et al.* (*2*) subsequently explained this effect by finding that the simultaneous feeding of this compound prevents the loss of microsomal dye-metabolizing enzymes which usually accompanies feeding with aminoazo dyes. Other investigations have shown that treatment in vivo with methylcholanthrene or benzpyrene increases a number of microsomal enzymatic activities assayed in vitro. These include N-demethylation (*3*), ring hydroxylation (*4*), aminoazo dye reduction (*3*), reduced triphosphopyridine nucleotide–cytochrome *c* diaphorase activity (*5*), formation of protein-bound aminoazo dye (*6*), and the incorporation of leucine-C^{14} into microsomal protein (*5*). The effect of treatment with methylcholanthrene, however, is not general to all microsomal enzymes, since some activities have been found to be either unaffected or decreased (*7*).

Many of the enzyme systems enhanced by these compounds are involved in drug metabolism. Conney and Burns (*8*) have shown that their enhanced activity has functional significance as indicated by an increased rate of drug metabolism in vivo and a shortened duration of drug action. For example, treatment of rats with 3-methylcholanthrene reduces zoxazolamine paralysis from 730 to 12 min, and phenobarbital, which was also found to enhance the activity of some microsomal enzyme systems (*8*), reduces the duration of meprobamate paralysis from 1356 to 175 min.

It has been suggested that the increased activities of the microsomal enzymes reflect an increase in their rates of synthesis (*3, 4, 6*). In order to examine this possibility further, a study was undertaken to determine the effects of two of these compounds, methylcholanthrene and phenobarbital, on the in vitro rate of amino acid incorporation into protein. Two cell-free amino acid incorporating systems were used. One consisted of mitochondria, microsomes, and cell sap, in which adenosine triphosphate (ATP) was generated by oxidative phosphorylation (system I). In the other, mitochondria and oxidizable substrate were replaced by a creatine phosphate-ATP generating system (system II). All experiments were performed on matched groups of control rats and rats treated with methylcholanthrene or phenobarbital. Each group consisted of four Sprague-Dawley male rats weighing 40 to 50 g. The methylcholanthrene treatment consisted of a single intraperitoneal injection of 1 mg of compound in 0.25 ml of corn oil. The phenobarbital treatment consisted of three daily intraperitoneal injections of 2.0 mg of sodium phenobarbital in 0.25 ml of water. Control groups were given equivalent amounts of solvent alone. In the experiments with methylcholanthrene the animals were killed 20 hours after injection; in the experiments with phenobarbital the animals were killed 1 day after the last dose. Animals were fasted for 20 hours before they were killed. The rats were decapitated, and the livers were removed and homogenized individually in 5.0 ml of ice-cold 0.25*M* sucrose per gram of liver. The homogenates from each group of rats were pooled, and mitochondria, microsomes, and supernatant fractions were separated by differential centrifugation (*9*). In system I the mitochondria and microsomes were suspended in 0.25*M* sucrose and mixed with supernatant fluid in proportions yielding mitochondria and microsomes equivalent to 200 mg and supernatant fluid equivalent to 67 mg of fresh liver per 0.9 ml, the aliquot of the mixture added per flask. In system II microsomes and supernatant fluid only were used, and these were so mixed that 0.6 ml, the aliquot of mixture per flask, contained microsomes equivalent to 200 mg and supernatant fluid equivalent to 67 mg of

Table 1. The effects of treatment with methylcholanthrene (MC) or phenobarbital on amino acid incorporation in vitro.

Treatment	Precursor	Specific activity	Effect (%)*
	System I†		
Control	DL-Leu-C^{14}	53.5	
MC	DL-Leu-C^{14}	85.9	+ 61 (8)
Control	DL-Leu-C^{14}	50.1	
Phenobarbital	DL-Leu-C^{14}	132.0	+ 163 (2)
	System II		
Control	DL-Leu-C^{14}	48.4	
MC	DL-Leu-C^{14}	64.1	+ 32 (2)
Control	sRNA-Val-C^{14}	56.6	
MC	sRNA-Val-C^{14}	64.9	+ 15 (1)
Control	sRNA-Pro-C^{14}	17.7	
MC	sRNA-Pro-C^{14}	21.6	+ 22 (2)

* The numbers in parentheses indicate the number of experiments.

† In system I each flask contained 20 μmole of potassium phosphate buffer, *p*H 7.4; 50 μmole of potassium α-ketoglutarate; 10 μmole of $MgCl_2$; 270 μmole of sucrose, 0.8 μmole DL-leucine-1-C^{14} (specific activity 5.47 μc/μmole), mitochondria and microsomes each equivalent to 200 mg of rat liver and supernatant fluid equivalent to 67 mg of rat liver. In system II each flask contained 20 μmole of potassium phosphate, *p*H 7.4; 270 μmole of sucrose; 40 μmole of creatine phosphate; 5.0 μmole of adenosine 5′-monophosphate; 0.5 μmole of guanosine 5′-triphosphate; 10 μmole of $MgCl_2$; 0.25 mg of creatine phosphokinase; microsomes equivalent to 200 mg of rat liver; and supernatant fluid equivalent to 67 mg of rat liver. The radioactive precursors in this system were either 0.8 μmole of DL-leucine-1-C^{14} (specific activity 5.47 μc/μmole), sRNA-L-proline-U-C^{14} (5.3 OD_{260} units containing 1262 count/min) or sRNA-L-valine-U-C^{14} (24 OD_{260} units containing 1063 count/min). The sRNA was prepared by the method of Cantoni (*13*), and the sRNA-amino acid was prepared by incubating sRNA with the appropriate C^{14} amino acid and activating enzyme (*14*). All flasks were prepared in duplicate and had a final volume of 1.7 ml. Incubations were at 37°C for 20 min. Reactions were stopped with 1.7 ml of 12 percent trichloroacetic acid, and the proteins were washed, plated, and counted as previously described (*15*).

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to contributors" [*Science* **125**, 16 (1957)].

Table 2. The amino acid incorporating activity of various recombinations of fractions from normal rats and rats treated with methylcholanthrene (MC).

Source of homogenate fraction			% of Control*
Mitochondria	Microsomes	Supernatant	
Normal	Normal	Normal	100
MC	Normal	Normal	122
Normal	MC	Normal	112
Normal	Normal	MC	125
MC	MC	Normal	150
MC	Normal	MC	152
Normal	MC	MC	144
MC	MC	MC	166

* The activity of the control flask containing all normal components was arbitrarily set at 100. Represents average of two experiments. Experimental conditions are the same as system I described in Table 1.

fresh liver. Incubation procedures are described in Table 1.

In the series of experiments with the system containing mitochondria (system I), administration of methylcholanthrene or phenobarbital resulted in average increases of 61 percent (eight experiments) and 163 percent (two experiments), respectively, in the rate of amino acid incorporation into protein (Table 1). In experiments with the microsomal-supernatant system (system II), treatment with methylcholanthrene resulted in an average stimulation of 32 percent (Table 1).

Although all the enzyme systems previously reported to be enhanced by administration of methylcholanthrene are microsomal, the effect on amino acid incorporation is not wholly accounted for by a difference in the microsomes. As is shown in Table 2, each of the homogenate fractions contributed to the increased incorporation rate observed in the preparations from the rats treated with methylcholanthrene. Thus amino acid incorporation was stimulated 12 to 25 percent when only one of the three fractions used was from the treated rats, 46 to 55 percent when two of the three were from treated rats, and 66 percent when all three were from treated rats. Conversely, the effect was reduced but not completely removed by the replacement of any one or two of the fractions from treated rats by the corresponding fractions from normal rats.

One of the later stages of protein synthesis involves the transfer of soluble ribonucleic acid bound amino acid (sRNA-AA) to microsomal protein (*10*). As shown in Table 1, this is at least one of the steps stimulated by treatment with methylcholanthrene.

The results of the present study demonstrate that treatment with methylcholanthrene stimulates amino acid incorporation into protein whether the amino acid is added free or bound to soluble ribonucleic acid (sRNA). These results, therefore, suggest that this treatment accelerates the rate of protein biosynthesis. Such an action is consistent with the hypothesis that the enhanced enzyme activities previously observed after treatment with methylcholanthrene result from an increased rate of enzyme synthesis. It is noteworthy that ethionine, which is known to inhibit protein synthesis (*11*), has also been observed to inhibit the stimulatory effect of methylcholanthrene on the enhancement of certain enzyme activities (*3*, *6*).

Confirmation of this hypothesis requires a demonstration of a net increase in newly synthesized enzymes. In the absence of such definitive evidence, alternative explanations of the effect of methylcholanthrene should be considered. It is possible that the increased rate of amino acid incorporation may not be causal to the increased enzymatic activities, but rather another example of a microsomal enzyme system enhanced by methylcholanthrene. A possible mechanism is an increased microsomal membrane permeability resulting in a greater accessibility of microsomal enzyme sites (*12*).

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26 April 1961

Nonlinear Property of the Visual System at Fusion

Abstract. The response of the visual system to intermittent stimulation at rates above the fusion point is generally considered to be independent of frequency, that is, linear with respect to time-average luminance. However, trains of 1000 light pulses per second alternated successively with trains of 500 light pulses per second may be perceived as flickering even though the time-average luminance is the same in each train.

Recent experiments (*1*) have indicated that the traditional critical flicker frequency (CFF) is at best a special case of a much more general class of visual phenomena relating to temporal visual acuity. A perceptually fused train of light pulses may be reduced to flicker by shortening the duration of every other "on-off" time in the train, an operation which preserves duty ratio and keeps average energy constant. Such an effect suggests that the nature of the visual response may be further illuminated by more extended use of this type of stimulation.

The research reported here involved trains of pulses such as shown schematically in Fig. 1. In this figure, the typical CFF configuration is represented by the top line; square wave pulses are presented successively, with "on" times equal to "off" times. The remaining three lines represent the type of stimulation used in the experiment; two different frequencies of light pulses (standard and variable) presented successively in a continuous sequence. The figure, drawn to scale and pictured as the stimuli appear on an oscilloscope, illustrates the effect obtained with one setting of the standard for three frequencies of the variable train of pulses. Note that, in all cases, each "off" time equals the preceding "on" time.

The standard and variable trains of pulses were generated by two independent circuits, each providing "on" and "off" triggers with continuously variable frequency. The circuits were designed so that duty ratio (proportion of "on" time) remained invariant at 0.5 with changes of frequency. Alternation of the standard and variable trains of pulses was accomplished by switching back and forth between the two circuits. Gating times were controlled by two cascaded time-delay units. The standard train of pulses was generated for a duration determined by the setting of one of the time-delay

units. When this unit shut off, the circuit generating the standard train was stopped, the circuit generating the variable train started, and the second time-delay unit switched on. The end of the second unit's delay stopped the variable circuit, started the standard circuit, and reset the first time-delay unit, and so on. Gating was accomplished with simple logic circuits so that neither the standard nor the variable train of pulses was ever interrupted in the middle of a period—that is, there was an integral number of "on-off" times in all trains of pulses.

The triggers generated by the above circuitry were fed to a d-c generating source to produce intermittent electric square waves which served as the input to a Sylvania R1131C glow-modulator tube. The tube illuminated a spot subtending ½° of visual angle with a luminance of approximately 2000 mlam. The luminance of the surround was approximately 22 mlam. All observations were made monocularly.

Gating times were set so that each train contained at least three pulses and had a duration of no less than 40 msec. For each series of measurements the period ("on" time plus "off" time) of the pulses in the standard train was set at a fixed value. The observer then adjusted the period of the pulses in the variable train until a point of fusion was reached. Settings of the standard train periods ranged from 1 to 18 msec. The output of a photocell illuminated by a second glow-modulator tube permitted continuous monitoring of the stimulus on a Tektronix oscilloscope during the course of the measurements.

The data for three observers are shown in Fig. 2. The coordinates have been labeled in terms of both frequency and period to facilitate interpretation. Each data point represents the mean of ten observations. Inspection of the data for observer DF will illustrate the procedure. With the period of the standard pulses set at 5 msec, the period of the variable pulses is reduced until a value is reached at which the light no longer appears to flicker, in this instance approximately 6.2 msec. The period is then further reduced until a value is reached at which flicker reappears, here approximately 2.6 msec. For this observer, then, with the frequency of the standard pulses set at 200 pulses per second (period, 5 msec), flicker is perceived when the frequency of the variable pulses is less than 161 or greater than

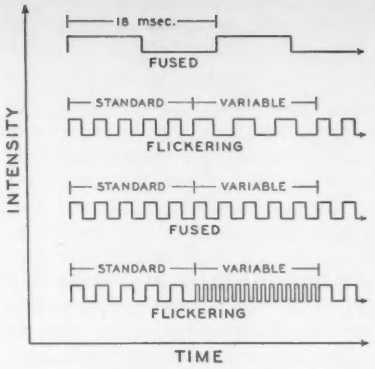

Fig. 1. Schematic representation of intermittent photic stimuli used in this study, drawn to scale.

385 pulses per second. The curves connecting the data points for each observer in Fig. 2 thus enclose a region of fusion. All points outside the boundaries of an observer's curve represent the periods (or frequencies) of alternating trains of pulses which are seen as flicker (the CFF for each observer is represented by the data point lying on the diagonal).

Although smooth curves were not drawn to fit the points, it seems clear that the best fitting curves would be, throughout their greatest extent, straight lines with a slope of +1, displaced by a constant amount from the diagonal. An estimate of this displacement for a given observer is the average absolute displacement of his data points from the diagonal. For observers, MB, DF, and PM, these values are approximately 0.52, 1.8, and 2.9 msec, respectively.

The curves in Fig. 2 were extrapolated to these values on the coordinates. Such an extrapolation estimates the values which might be obtained by alternating d-c with high-frequency intermittent stimuli of the same time-average luminance. Figure 2 shows further that, for observers DF and MB, flicker is perceived when a train of 1000 pulses per second is alternated with a train of 500 pulses per second. Both trains of pulses, however, have the same time-average luminance and, when viewed in isolation, appear fused. These data do not invalidate the Talbot-Plateau law, but serve only to emphasize that brightness, in the context of this law, implies "steady-state" brightness. Abrupt changes of frequency while time-average luminance remains

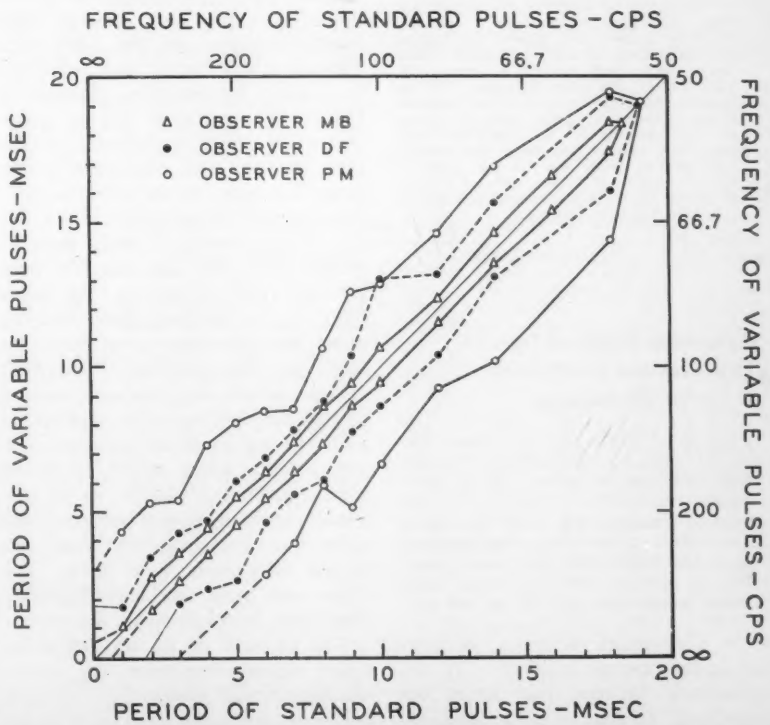

Fig. 2. Fusion as a function of the periods of the pulses in alternating trains of intermittent photic stimuli. Each symbol represents the mean of ten observations.

constant seem to induce an effect analogous to the Broca-Sulzer effect. The linearity of the function with respect to period, however, argues that changes of temporal pattern, rather than frequency, are responsible.

A current model developed by de Lange, Kelly, and Levinson (*2*) regards the visual system as selectively attenuating the high-frequency components of intermittent stimulation. Such a treatment relies on the linearity implied by the "steady-state" brightness of the Talbot-Plateau law. The data presented in this paper clearly indicate the limited generality of such a model (*3*).

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1 May 1961

Differential Acuity of the Two Eyes and the Problem of Ocular Dominances

Abstract. While it has long been thought that no relationship exists between the eye of greater visual acuity and the eye favored in sighting, the data collected suggest the need for a re-examination of this issue. Sighting-dominance and acuity-dominance were associated. In addition, most individuals tested showed acuity-dominance of the left eye.

In a systematic discussion of types of ocular dominance, Walls (*1*) differentiated between two which are commonly considered to be independent, namely, sighting-dominance (one eye is consistently aligned with a near point when sighting or pointing at a far point) and acuity-dominance (one eye has greater visual acuity than the other). In this laboratory both kinds of dominance were measured in a large sample of college students. This paper reports the frequency of left versus right acuity-dominance and the association of sighting-dominance and acuity-dominance.

Table 1. The association of sighting-dominance and acuity-dominance in 319 college students.

Acuity-dominance	Right sighting-dominance	Left sighting-dominance
None	66	21
Right	76	23
Left	80	53

Visual acuity was measured with a Bausch & Lomb Ortho-Rater, which measures far acuity and near acuity separately (*2*). For two subject groups one eye shows greater acuity than the other, that is, acuity-dominance. These groups consist of subjects in which (i) one eye shows greater acuity at both far and near viewing distances and (ii) the eyes show equal acuity at one distance, but differential acuity at the other distance.

In the sample, acuity-dominance of the left eye was more frequent than acuity-dominance of the right eye. This surprising finding was supported by further investigation. Bausch & Lomb, Inc., reported that in many studies in which Ortho-Rater scores were obtained the best mean acuity score has been in the left eye (*3*). Since testing of the acuity of the left eye follows testing of both eyes together and of the right eye (for both far and near conditions), the possibility of a practice effect favoring higher left eye acuity scores exists. To investigate this possibility, I tested 80 college students, with sequence changed so that the right eye tests were last in each viewing condition and, therefore, any practice effect would favor the right eye. In this sample, as in previously tested ones in which the usual order was used, mean left acuity was greater than mean right acuity, and there were more left acuity-dominant than right acuity-dominant subjects.

The question of an association between sighting-dominance and acuity-dominance was studied by Gahagan, who used a different measure of visual acuity (*4*). While he concluded that the two dominances are independent, a trend appeared in his data which could support the opposite conclusion. For those subjects showing acuity-dominance, of 63 right sighting-dominant subjects 54 percent were right acuity-dominant, and of 21 left sighting-dominant subjects 62 percent were left acuity-dominant. A re-examination of Gahagan's published data taken in conjunction with the data from my own studies suggests an association between the two ocular dominances. Table 1 presents the number of subjects showing each combination of acuity-dominance and sighting-dominance. A χ^2 test of independence was made ($\chi^2 = 9.59$; $p < .01$).

Inspection of Table 1 clarifies the finding of a prevalence of left acuity-dominance. Among right sighting-dominant subjects, right and left acuity-dominance are equally common. However, among left sighting-dominant subjects, left acuity-dominance is considerably more frequent than right acuity-dominance.

These data imply that to the extent that a sample consists of left sighting-dominant subjects, left-acuity would occur in more than half the sample. Further, the data imply that in studies in which there is monocular viewing with the possibility of acuity being relevant, an index of the differential acuity of the two eyes should be obtained, since it can no longer be assumed that left and right acuity-dominance will be equally distributed. In order confidently to generalize from these results it is suggested that (i) populations other than college students be sampled, and (ii) indices of acuity other than Ortho-Rater scores be obtained.

The clear demonstration of a bias favoring left acuity-dominance related to sighting-dominance may assist in the development of a valid conceptualization of ocular dominances and related perceptual events. The basic question of the origin of ocular dominances remains unanswered, and can be expected to remain so barring developmental studies.

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5 May 1961

Repeatability and Standardization in Cattle Blood Typing

Abstract. Thirteen laboratories engaged in cattle blood typing have participated in tests to compare their blood-typing reagents. Over 75,000 individual tests of cattle erythrocytes were made. When the laboratories tested duplicate samples without knowing they were duplicates, the results were in agreement 99 percent of the time. Tests on samples from the same 199 animals with 56 different reagents from two or more of the laboratories showed agreement 89 percent of the time. The agreement was unexpectedly good, since the primary purpose of the tests was to allow comparison of independently developed reagents and since the sources of error were numerous.

The last two decades have witnessed great progress in the study of blood groups in cattle (see *1* for a review). New discoveries intensified the problems of standardization and nomenclature for cattle blood-group workers around the world. In March 1956, a cooperative program of comparative testing was initiated by the Dairy Cattle Research Branch of the U.S. Department of Agriculture to aid workers in this field. Blood samples from 40 cattle were sent to the participating laboratories for concurrent testing. Thus each laboratory had the opportunity to compare its findings with those of others testing blood from the same animals. Six trials have been completed, and over 75,000 individual tests on the red blood cells of cattle of various breeds were made. Laboratories (*2*) in Canada, Denmark, Finland, Holland, Norway, South Africa, Sweden, the United States (California, Ohio, Wisconsin, and Wyoming), and West Germany (Göttingen and Munich) participated. Laboratories in Belgium and France will join the program soon.

Primarily, the program facilitates comparison of blood-typing reagents and genetic studies. It has also provided data on the repeatability of cattle blood-typing tests. This report is a summary of the results of the analysis of repeatability. A more detailed account will be published elsewhere (*3*).

Two aspects of repeatability were examined: (i) the agreement within laboratories when they unknowingly tested samples of blood from the same animal twice; and (ii) the agreement among laboratories when they tested the same cells with the same reagents (reagents produced independently and presumed to detect the same antigenic factors). Some samples were repeated within trials and others between trials. Samples from 41 animals were repeated during the course of the six trials. There was an opportunity for agreement each time a laboratory tested repeat samples with the same reagent. Table 1 summarizes the results of the repeat sample tests with laboratories designated by letter codes. The high repeatability of 99 percent is striking. It is obvious, however, that laboratory G experienced a great deal of difficulty, and over-all means are shown with and without the inclusion of that laboratory's results. To estimate agreement among laboratories, reports of reactions in the blood-typing tests were studied within reagents and trials. For example, in a particular trial, if the participants used a reagent which was supposed to detect the same blood factor, then the results with that reagent were compared to determine the number of samples of the 40 tested on which they agreed. The cells either had the blood factor in question (+ reaction) or did not (− reaction). An agreement was recorded when all laboratories testing a particular sample reported the same result. Results were compiled for 56 different reagents which had been used in one or more trials by some or all of the laboratories.

The over-all percentage of agreement among laboratories was 82.1. When the results of laboratory G were excluded, this figure became 89.0 percent, indicating that G was responsible for much of the disagreement. The analysis was carried further and results were calculated without including those from whatever laboratory caused the most disagreement in each trial-reagent group. The term "trial-reagent group" refers to the results reported by all the laboratories in which a certain reagent was used in a particular trial. With this procedure the over-all mean was 94.6 percent agreement. These results indicate that one laboratory was usually responsible for most of the disagreement in each group. This is not unexpected since the purpose of the trials was to facilitate comparison of independently developed reagents.

These results are a tribute to the accomplishments of the workers in the relatively new field of cattle blood grouping. The high degree of repeatability is especially noteworthy when the numerous sources of error are considered. The program requires a great deal of transcription of results, and some transcription errors have occurred. Although some of these were discovered by the laboratories after reports were submitted, no corrections for them have been made. Occasionally the blood arrived at a laboratory in poor condition due to delay en route. This was particularly true in two instances, but all the results were included since there was no objective way to pick and choose among them. There is no doubt, however, that the tests were not as accurate as they would have been if some of the samples had not deteriorated. Also included are the technical errors inherent in any complex test, such as the hemolytic test, which involves thousands of tubes to which red blood cells, reagents, and complement are added a

Table 1. Summary of results of tests on repeat samples* by laboratory.

Laboratory	Between trial repeats†				Within trial repeats‡				All repeats
	No. of animals	No. of possible agreements§	Actual No. of agreements	Percent of agreements	No. of animals	No. of possible agreements	Actual No. of agreements	Percent of agreements	Percent agreement (weighted mean)‖
A	26	1398	1388	99.3	15	768	768	100.0	99.5
B	0	0	0		3	54	52	96.3	
C	7	168	167	99.4	6	156	156	100.0	99.7
D	13	480	473	98.5	12	486	484	99.6	99.1
E	13	494	483	97.8	12	468	468	100.0	98.9
F	13	538	522	97.0	12	528	526	99.6	98.3
G	11	273	213	78.0	9	249	219	88.0	82.8
H	5	296	292	98.6	9	549	549	100.0	99.5
I	5	181	168	92.8	9	405	403	99.5	97.4
J	0	0	0		3	108	107	99.1	
K	0	0	0		3	111	111	100.0	
Unweighted means¶									
Over-all				95.2				98.4	96.9
Without G				97.6				99.4	98.9
Weighted means ‖									
Over-all				96.7				99.0	97.9
Without G				98.2				99.8	99.0

* Repeats: duplicate samples sent to the laboratories without their knowledge. †Samples from the same animal included in two different trials. ‡ Samples from the same animal included twice in the same trial. § Opportunity for agreement occurred whenever a sample was tested twice with the same reagent. (Reagents considered by a laboratory to be experimental were not included.) ‖ Over-all number of agreements as a percentage of over-all number of possible agreements. ¶ Mean of laboratory means.

drop at a time. Still another source of error—and the main reason why these comparison tests are so useful to any one of the blood-group workers—is the limited choice of animals and especially breeds, which is available to most of the workers for production and standardization of reagents.

These results indicate the accuracy with which blood types are regularly determined in cattle when the tests are made by experienced persons. They also indicate that these trials are helpful in solving the problems of standardization. Comparison of new reagents, developed independently in different laboratories, often indicates that some are detecting the same antigenic factor and leads to an agreement on nomenclature. The trials will be continued on an annual basis.

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20 April 1961

Some Characteristics of a Thermophilic Blue-Green Alga

Abstract. An alga identified as *Synechococcus lividus* has an exponential growth rate of nine doublings per day at 52°C with illumination of 1500 foot-candles. It uses nitrate or urea as a nitrogen source and does not use acetate or glucose. It seems a promising organism for atmospheric regeneration in sealed cabins.

Photosynthesis by algae is one of the most promising methods for atmospheric regeneration in sealed space cabins. In order for such a system to be practical, the algae used must be capable of achieving high growth and photosynthetic rates. We wish to present some data on a thermophilic blue-green alga that appears to have value in this respect.

The organism was originally obtained by the USAF School of Aviation Medicine in a collection of mixed specimens from the hot springs in Yellowstone National Park. It was received in our laboratory in a mixed culture of algae and bacteria and was isolated in unialgal culture by serial transfer in liquid cultures maintained at a temperature between 50° and 55°C. The cells are about 1.4 μ in diameter and 4 to 9 μ long, the most common length being about 6 μ. They are straight or slightly curved. Some occur in pairs joined at the ends. Polar granules are occasionally observed. From the description given by Copeland (see *1*) this species has been tentatively identified as *Synechococcus lividus*.

A nitrate medium described by Gafford and Craft (*2*) gives good growth and was used for all experiments reported here. Best growth occurs when the *p*H of the medium is adjusted to about 7.5. We have recently found that a urea medium (*3*) recommended for the culture of the thermophilic strain of *Chlorella pyrenoidosa* will also provide maximum growth at *p*H 7. There is no growth in the dark when acetate or glucose is provided as the carbon source.

Growth was measured by determining the optical density of the suspension at 500 mμ with a Beckmann model DU spectrophotometer, using 1-cm cells. Agreement between optical density and packed cell volume was best when the suspension was diluted to keep the measured optical density below 0.25. Thus the measurements were all made in much the same density range. The measured density was multiplied by the dilution factor to express the cell concentration of the culture.

Cultures of 100 ml were grown in test tubes suspended in thermostatted water baths. The bath containers were glass jars 12 in. in diameter, each supported over a grid of ten 15-watt fluorescent lamps. The bottoms of the tubes were about 12 cm above the lamps. Carbon dioxide was provided by bubbling 2 percent carbon dioxide through small polyethylene tubes. We have estimated the effective light intensity, by measurements with a Weston model 614 footcandle meter, to be about 400 ft-ca. Other cultures were grown in the same tubes at higher light intensity. These tubes were placed in a narrow water bath sandwiched between two vertical panels, each of which held eight 107-watt Powergroove fluorescent lamps. Here the effective light intensity is about 1500 ft-ca.

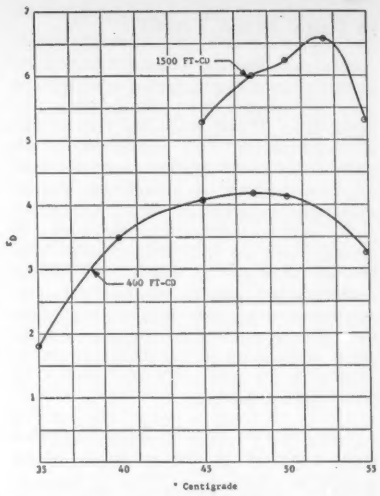

Fig. 1. Growth rate of *S. lividus* in doublings per day (r_D) with respect to temperature at 400 and 1500 ft-ca illumination.

Figure 1 shows the growth rate at various temperatures for both light intensities. The general shape of the curves is rather similar to those shown for *Chlorella pyrenoidosa* TX 71105 (*4*). With the higher intensity light the cells did not tolerate temperatures below 40°C as well as with less light. The optimum temperature is higher with more light. Growth occurs even at 60°C, but at a lower rate.

The curves of Fig. 1 were determined by measuring growth over periods of 21 to 23 hours. Since it appeared that the cultures were light-limited at the end of that time, we measured density at shorter intervals. Exponential growth rates determined in this manner fell in the range of 6 to 7 doublings per day at 400 ft-ca and 8 to 9 doublings per day at 1500 ft-ca for periods of 7 hours or more. The shift to linear growth occurred after 12 to 14 hours. At this time the calculated cell density is 0.4 and 1.8 ml of cells per liter at low and high intensity light. Thereafter growth continued at about 0.09 and 0.3 ml of cells per liter per hour. As expected, the linear growth rates are in approximately the same ratio as the light intensities.

We find this alga of particular inter-

est for atmospheric regeneration systems. It has practically no tendency to cause foaming or to stick to the surface of culture vessels, even in old or mismanaged cultures. The higher culture temperature results in more efficient cooling systems, while discouraging fungal and bacterial contamination (*5*).

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4 May 1961

Localization Effects with Steady Thermal Noise in One Ear and Pulsed Thermal Noise in the Other

Abstract. When the duration or repetition rate of pulses in the left ear is increased, while steady, in-phase, thermal noise sounds in the right ear, the pulses are heard to move toward the median plane. At still longer durations (for a given repetition rate) the loudness of noise on the right diminishes, until finally all sound is localized at the median plane.

In these experiments thermal noise was led through a mixing circuit such that part went through an electronic switch and interval timer (Grason-Stadler) to the left ear, and the other part went to the right ear. Thus pulses were presented to the left ear, and steady, in-phase noise to the right ear. Over-all presentation time of stimuli to both ears for a given judgment was set at 10 sec by a Hunter interval timer. Signals in each channel went through an attenuator and transformer before arriving at the earphone (Telephonic TDH-39).

Perception corresponding to left ear pulses. The left ear attenuator was set at 40 db above threshold for each subject. (Thresholds were obtained for 200 msec, 1 per sec bursts of noise.) The right ear attenuator was then set to give voltage into the right phone equal to that in the left phone. Repetition rates of pulses used were 1.4, 4.7, 13.9, 58.8, and 105.3 per second. For a given pulse repetition rate, the duration of the pulse could be increased until the subject heard the pulses at the median plane. (The median plane is defined as the plane passing between the cerebral hemispheres.) Thresholds for such centering were obtained by the Method of Limits, using four crossings.

Figure 1, curve *A*, shows data (medians) for the ten subjects used. The duration required to center the pulses decreases as pulse repetition rate is raised. Individual differences in required duration are more marked at low pulse rates, but all subjects tended to require increasingly smaller durations with increasing repetition rates. (A Friedman rank order test shows significance beyond the .001 level.)

The greatest interaction occurs when the noise in the two ears is in phase (perfectly correlated); for in another experiment with an additional ten subjects, we found that reversing the phase of noise coming to the two ears significantly raised the duration required to center the pulses. It should be noted that Pollack (*1*) did not obtain significant localization effects by using partially correlated noise.

The present results can be said to show summation effects, since increasing pulse duration increases center localization effects. Tobias and Schubert (*2*) also found summation effects in overcoming an initial binaural transient disparity. They suggest that the power of the initial transient disparity on localization may be related to neural onset responses. It is possible that in the present experiment the localization "power" of the neural onset response in the left ear must be overcome by increasing pulse duration and thus increasing duration of interaction with the right ear stimulus.

Right ear perception. The duration of the *gap* in the left ear noise was increased until the subject could first detect noise at the right ear position. Thresholds for this detection were determined by the Method of Limits, with four crossings. Half of the subjects obtained these thresholds before those

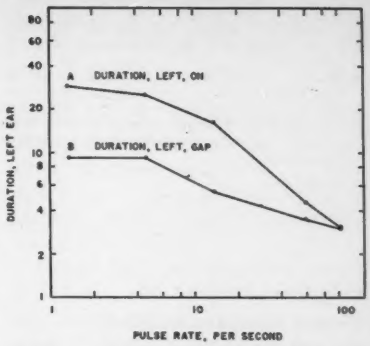

Fig. 1. (Curve *A*) Median duration (left ear) required by subjects to center the pulses, decreasing as the pulse repetition rate is raised. (Curve *B*) Median duration of gap (left ear) required by subjects for the detection of noise in the right ear, decreasing as the pulse repetition rate is raised.

described previously. Order of conditions, as in the preceding experiment, was determined for each subject from a set of randomly drawn numbers.

Figure 1, curve *B*, shows that the median duration of gap required for the detection of noise in the right ear falls for the higher repetition rates used. (A Friedman rank test shows significance beyond the .001 level.)

Our tentative interpretation of these results is as follows: Conduction through the neural channel corresponding to right ear localization perception is suppressed when the stimulation occurs in the left ear. When the left ear stimulus ceases, nerve impulses can pass through this channel; but beyond this point is a summation process, because of which detection depends on repetition rate and duration.

Questioning of the subjects revealed that the right-ear signal was heard to increase in *loudness* as gap duration was increased. Furthermore, at low repetition rates (1.4 and 4.7), the right-ear signal was heard typically as pulsing; at higher rates the signal heard at the right ear location was heard as *continuous*. These latter continuity effects (see *3*) may occur over such long periods of time because of the facilitating effect of the continuous input to the right ear. A possible mechanism might be an additional neural "facilitation" circuit from a region prior to a localization gating mechanism to a region beyond (*4*).

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6 April 1961

Polymeric Particles of Protein Insoluble at pH 5 from Rat Liver

Abstract. A procedure is given for approximating the volumes of small particles of protein insoluble at *p*H 5. Among particles smaller than 2.08×10^7 cubic angstroms the change in size was linear. The distribution of sizes indicated a polymeric relationship among the particles.

Approximately 25 percent (*1*) of the soluble protein fraction (*2*) from rat liver is insoluble at *p*H 5. When fixed in OsO_4 and viewed in an electron microscope, this insoluble protein has some of the dimensional characteristics of the endoplasmic reticulum (*3*). Because of the presumed role of the ergastoplasm (*4*) in the synthesis of cellular end products and the relations of the microsomal fraction of cells to this organelle, we examined the further possibility that the soluble proteins might serve as a source of α and γ cytomembranes (*5*). This study was primarily concerned with the distribution of the sizes of the smallest particles derived from the insoluble protein of the "soluble protein fraction" of liver cells.

Rats were killed by decapitation, and the livers were immediately removed and placed in cold *p*H 7.12 phosphate-buffered 0.25*M* sucrose solution (2.5 ml of sucrose solution per gram of tissue). Homogenization was begun within 10 min post mortem. At 20 min post mortem the homogenate was spun in the centrifuge at 20,000*g* for 90 min to remove mitochondria, and 2 days after that it was spun at 102,000*g* for 70 min to remove the microsomal fraction. The *p*H of the resulting supernatant was adjusted to 5.07 with 0.10*N* HCl. The precipitate that formed promptly was spun down to give a pellet of protein insoluble at *p*H 5.

Only a slight amount of precipitate developed from the 20,000*g* (mitochondrial) supernatant in the 2 days preceding the final spin. It was assumed, therefore, that the Mg^{++} concentration was high enough (*6*) to prevent clumping of the cytomembranes and that a normal microsomal fraction could be removed from this sample by centrifugation. We have found that the precipitation of this insoluble protein from a mitochondrial supernatant also removes the microsomal fraction. However, because this insoluble protein from the soluble protein fraction, prepared as described, had none of the electron-dense characteristics (*4*) of the microsomal fraction, we considered the precipitate obtained to be essentially free of microsomes.

The pellet of protein was suspended in 10 ml of a 1-percent OsO_4 solution at *p*H 5.04. The fixed pellet was triturated by using a glass grinder and resuspended in a test tube. Those particles small enough to remain in suspension after 24 hours in the OsO_4 were examined in the electron microscope. Small drops of the suspension were dried on collodion-covered RCA stainless steel grids in a desiccator for 1 week. The grids were shadowed with chromium at an angle of 5 : 1 (tan $\angle$ $= 0.2$). The particles on the grids were viewed and photographed in a Philips model EMU electron microscope. A magnification of 40,000 was the most useful for measuring the dimensions of the particles.

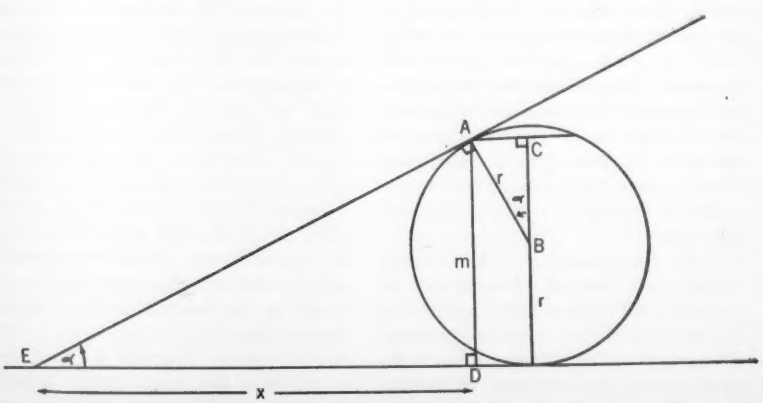

Fig. 1. Diagram of hypothetical particle upon which were based calculations of the volumes of the particles of protein insoluble at *p*H 5.

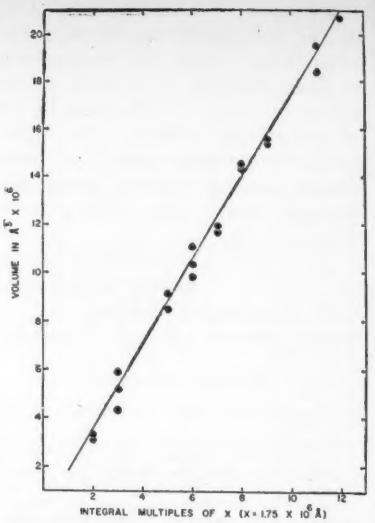

Fig. 2. Plot of the particle sizes of protein insoluble at *p*H 5, expressing the linearity of the volumes with respect to integral multiples of the smallest volume.

Our measurements indicated that the particles were not spherical, but ellipsoid. By assuming that they are probably flattened ellipsoids, useful presumptive calculations of the volumes of the particles are possible. A further assumption, obviously inaccurate but reasonable for the purposes of such determinations, was that the beam of chromium was perpendicular to the major axis of the ellipsoids. A fair approximation of the volume of the particles was calculated in the following manner (Fig. 1):

Given that $\tan \alpha = 0.2$, and by measuring the length of the shadow, X, the apparent or measured height of the particle, M, can be calculated as

$$M = X \tan \alpha$$

By construction, $\triangle ADE \sim \triangle ABC$, therefore, $\angle DEA = \angle ABC = \alpha$. From this, it is apparent that

$$M = R + (R \cos \alpha)$$

where R is the radius of the hypothetical sphere and

$$R = M/(1 + \cos \alpha)$$

The formula for the volume of an ellipsoid is $V = 4/3\pi AB^2$ where A is the length of the semimajor axis and B is

the length of the semiminor axis which is equal, in these calculations, to R.

Since we can measure A directly, the expression for the volume of the particles is

$$V = 4/3\pi\ AR^2$$

A population of 34 particles was measured from a typical field, and the volumes of each of the particles were calculated. Five arbitrary classes of particle size were designated. The minimum, maximum, and mean values for the length ($=2A$), height ($=2R$), and volume were tabulated for each of the particle classes (Table 1).

A further analysis was performed as follows. It was postulated that the three smallest particles might be "monomers" or, more correctly, small integral polymers of a monomer of protein insoluble at *p*H 5. The average volume of these three particles was 1.75×10^6 A^3 and was designated as X. The hypothetical polymers of this assumed "monomer" were then calculated and are represented by the straight line in Fig. 2.

Figure 2 shows the linearity of the distribution of the particles of protein at *p*H 5 whose volumes were experimentally determined. Despite the errors inherently involved in the measurements and calculations of the volumes of the particles, the suggestion that they are small integral values of some "monomer" is correct. Class V particles (Table 1) were not included in Fig. 2 because the probability for linearity increases with larger multiples of X (*7*). Figure 2 also indicates that errors remaining in the calculation of volume are linear with respect to the actual volumes. The polymeric nature of the small particles of the protein strengthens the possibility that this material could have the dimensions of intracellular membranes and could contribute to their formation. We reason, too, that localized intracellular changes in *p*H likely exert a profound influence on the formation of membranes from the "soluble protein" fraction. The size classes of the particles, as presented in this report, have no definable biological reality, other than those implied above.

Table 1. Particle sizes of insoluble protein from soluble fraction of rat liver.

Particle class	Min.	Max.	Mean
	Height (A)		
I	68	145	105
II	148	194	164
III	171	194	184
IV	201	223	212
V	240	308	281
	Length (A)		
I	424	791	576
II	622	791	740
III	734	961	833
IV	828	993	883
V	960	1545	1184
	Volume (10^6 A^3)		
I	1.642	5.994	3.393
II	8.419	11.95	10.26
III	14.28	15.03	14.73
IV	18.47	22.76	20.58
V	28.95	72.75	50.02

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25 April 1961

Transfer of Allergic Encephalomyelitis by Lymph Node Cells in Inbred Guinea Pigs

Abstract. Severe or lethal allergic encephalomyelitis was transferred between histocompatible guinea pigs by lymphoid cells capable of persistence in a viable state in the recipient. For optimum induction of passive disease, cells must be transferred before the day on which the disease becomes manifest in the donor.

Experimental allergic encephalomyelitis, induced in animals sensitized with brain or spinal cord preparations (*1*), was first passively transferred between parabiotic rats by Lipton and Freund (*2*). The disease has not been passively induced by transfer of anti-central nervous system serum or sensitized cells to normal random-bred animals, but has been transferred with lymphoid cells in "tolerant" rats by Paterson (*3*). Koprowski *et al.* (*4*) found histological evidence of passive sensitization in a small percentage of transfers between inbred rats (4 of 48 recipients showed damage in the central nervous system). The percentage of successful transfers was higher when the donors were splenectomized before lymph node cells were transferred (*5*). Since this disease is widely considered to be associated with hypersensitivity of the delayed type, and since the guinea pig is the animal of choice for studies in this area, in the experiments described in this report guinea pigs of the Wright (*6*) histocompatible (*7*) strain 13 were used for passive transfer of allergic encephalomyelitis. The facility with which transfers of this type of hypersensitivity are accomplished within this strain of guinea pig [Chase (*8*); Bauer and Stone (*9*)] is attributable to the viability of the lymphoid transplant in the recipient animal.

Except for cases otherwise recorded in Table 1, adult male strain 13 guinea pigs were sensitized by a single dose of strain 13 brain or spinal cord emulsified in complete Freunds adjuvant (*10*) injected intracutaneously into multiple sites in the nuchal region (0.25 ml of a 50 percent suspension of spinal cord or brain in 0.25 ml of Arlacel-Bayol containing 2.5 mg killed *Mycobacterium tuberculosis*). These donor animals were killed 5, 8, 11, or 12 days after injection, and the lymph nodes draining the nuchal region were removed; cell suspensions were then prepared and transferred by injection into the peritoneal cavity of normal recipients, as previously described (*9*). The donor : recipient ratio was roughly 2½ : 1. Some of the recipients were skin-tested with purified protein derivative (PPD) of tuberculin 14 to 23 days after transfer. Random-bred guinea pigs of the Hartley strain were used as control recipients. Donor and recipient animals were weighed each day to determine the onset of disease.

Table 1 shows that the allergic encephalomyelitis induced by isologous brain or spinal cord can be transferred between strain 13 guinea pigs and that the transfer results in severe or lethal disease in a large percentage of the recipients. In groups receiving cells 5, 8, or 11 days after active sensitization of donors, 17 of 20 strain 13 and none of 10 Hartley guinea pigs had the disease. In confirmation of Chase's prediction (*11*) and of Koprowski's results with inbred rats (*4*), the transfer was more likely to result in passive disease when the cells were taken before the symptoms were apparent in the donor (transfer at 5 and 8 days after active sensitization). Eleven days could elapse between sensitization and the successful transplantation of lymphoid cells from guinea pigs actively sensitized with brain preparations; but guinea pigs sensitized with the more potent spinal cord preparations were frequently manifestly ill by this time, and transfers at 11 or 12 days from these animals were not made under

optimum conditions. Early transfers insure that ample numbers of sensitized cells are still in the lymph node. Transfers made soon after the sensitizing injection are feasible in histocompatible guinea pigs because the cells are viable and the sensitization process can continue in recipients (*9*). The time interval between transfer and onset of disease in recipients was 7 to 10 days whether the transplant was made 5, 8, or 11 days after active sensitization of donor.

Several recipients receiving the cells from 2½ donors sensitized with the less potent brain preparation had a more severe form of the disease than actively sensitized animals from the group used for donors. Allergic encephalomyelitis in the recipients was not induced by active sensitization caused by traces of adjuvant-antigen removed with the lymph nodes, as is shown by the absence of signs of the disease in the Hartley recipient controls. Hartley guinea pigs have been shown to be of highest susceptibility to actively induced allergic encephalomyelitis (*12*); furthermore, this line is as susceptible as, or more susceptible than, strain 13 animals whether sensitized by intradermal or intraperitoneal route (*13*). When transfers were made at 5 days, although the disease was not apparent in control Hartley recipients, sensitization to tuberculin, probably of the active type, did occur, as was shown by strong reactions to purified protein derivative in the Hartley controls. In Hartley guinea pigs, 0.02 mg of *Mycobacterium tuberculosis* is a threshold dose in the emulsion used for induction of allergic encephalomyelitis, but only 1 μg of the killed and dried mycobacteria is required to induce strong sensitivity to purified protein derivative (*14*). Transfers at 8 to 12 days conferred weak active or possibly weak passive sensitivity to purified protein derivative to Hartley recipients (also see *9*). The eliciting of a tuberculin reaction in recipients of lymph node cells harvested 8 to 12 days after active sensitization, which would otherwise serve as a control for the successful transfer and persistence of viable lymphoid cells (*9*), was complicated by probable impairment of the skin reactivity of animals sick with encephalomyelitis.

The problems which come to the fore concerning lymphoid transfers between histocompatible guinea pigs were discussed previously (*9*, *15*).

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Table 1. Transfer of allergic encephalomyelitis. PPD = purified protein derivative of tuberculin; ND = not done; ? = doubtful reaction. Braces ( ) encompass animals receiving a given pool of cells.

Cell pool	Strain of recipient	Day of transfer	Cells (ml)*	Disease		Day of death	PPD test	
				Day of onset†	Degree‡		Day	Square of radius at 24 hr (mm²)
				Brain antigen				
I	13	11	2.0	10	++		22	72
I	13	11	2.0	10	++		22	61
I	Hartley	11	2.0		0		22	30
I	Hartley	11	2.0		0		22	?
II	13§	11	2.0	8	++		22	106
II	13§	11	2.0	9	++		22	121
III	13§	11	2.5		0		20	64
III	13§	11	2.5	10	++++	17	ND	
				Spinal cord antigen				
IV	13	12	2.0	10	+		14	68
IV	13	12	2.0		0		14	64
V	13	11	1.8	9	++		ND	
V	13	11	1.8	10	+		23	25
VI	13	8	1.7		0		17	42
VI	13	8	1.7	9	+++		17	?
VI	13	8	1.7	8–9	++++	12	ND	
VII	13	8	2.0	8	++++	14	ND	
VII	13	8	2.0	10	+++		17	49
VIII	13	8	2.0	8	++		16	28
VIII	Hartley	8	2.0		0		16	36
VIII	Hartley	8	2.0		0		16	49
IX	13	8	2.4		0		20	40
IX	13	8	2.4	9	++++	16	ND	
IX	Hartley	8	2.4		0		20	25
IX	Hartley	8	2.4		0		20	69
X	13	5	1.3	8	++++	13	ND	
X	13	5	1.3	7	++++	10	ND	
XI	13	5	0.8	7	++++	10	ND	
XI	13	5	0.8	8	++++	11	ND	
XI	Hartley	5	0.8		0		20	117
XI	Hartley	5	0.8		0		20	100
XII	Hartley	5	1.0		0		16	105
XII	Hartley	5	1.0		0		16	46
				Egg albumin antigen				
	13	8	1.8		0		18	110
	13	8	1.8		0		18	110
	13	8	1.8		0		18	106

* Packed lymph node cells. † Day after transfer that weight loss began. ‡ ++++, death; +++, paresis or paralysis; ++, impacted feces, wet mouth, loss of weight; +, loss of weight only. § Received 5 mg of *M. tuberculosis* and 0.5 ml of 50 percent brain suspension in 0.5 ml of Arlacel-Bayol (2× dose).

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27 April 1961

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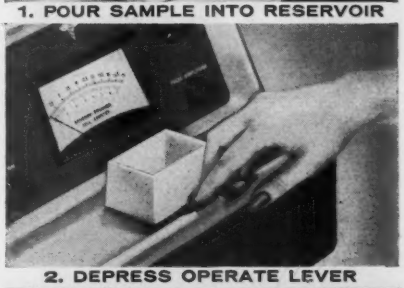
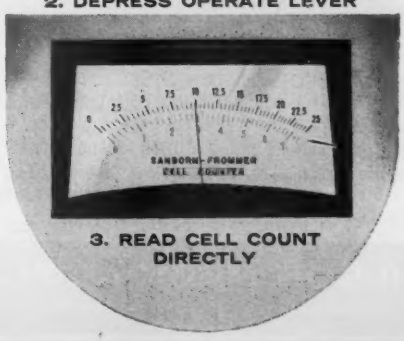
1. POUR SAMPLE INTO RESERVOIR



2. DEPRESS OPERATE LEVER



3. READ CELL COUNT DIRECTLY

Meetings

Respiratory Tract Diseases

An international congress on respiratory tract diseases of virus and rickettsial origin was held in Prague, Czechoslovakia, from 23 to 27 May 1961. The congress was under the sponsorship of the Czechoslovak Medical Society of J. Ev. Purkyně and the Czechoslovak Academy of Sciences. Karel Sedlacek and K. Raska were secretary-general and president, respectively, of the congress. There were delegates from Argentina, Brazil, Czechoslovakia, France, Germany, Great Britain, Hungary, Italy, the Netherlands, Poland, Romania, the Soviet Union, and the United States.

The papers presented dealt with etiology and pathogenesis of virus and rickettsial diseases of the respiratory tract, epidemiology and prevention, and clinical problems. They were of good quality and indicated a keen interest in viral and rickettsial respiratory infections on the part of scientists of Eastern Europe as well as scientists of Western Europe and the countries of the Western Hemisphere. The subject matter ranged from basic problems of virus composition, virus-cell relationships, and factors of specific and nonspecific immunity to the discovery and evaluation of the importance of new respiratory viruses and development of means for controlling infection. Finally, there was considerable discussion of the clinical consequence, in man, of infection with adenoviruses, influenza, and the rickettsiae.

Respiratory viruses and rickettsiae occur throughout the world, with little regard for geographic boundaries. It is important, therefore, that there be maximum exchange of information among scientists of all countries concerning them. The congress in Prague was the first truly international conference on this important health problem. It is to be hoped that this pioneering conference is but the first in a series of congresses on viral respiratory disease, with others to be held throughout the world in the future.

MAURICE R. HILLEMAN
(for the American delegation)
Merck Institute for Therapeutic Research, West Point, Pennsylvania

Forthcoming Events

September

19–22. Australian Conf. on Food Technology, Homebush (near Sydney), Australia. (T. B. Partridge, Australian Scientific Liaison Office, 1907 K St., NW, Washington 6)

19–29. International Conf. on Fish Nutrition, Washington, D.C. (FAO, Intern. Agency Liaison Branch, Office of the Director General, Viale delle Terme di Caracalla, Rome, Italy)

20–21. Industrial Electronics, symp., Boston, Mass. (W. M. Trenholme, General Electric Co., West Lynn, Mass.)

21–22. Air Pollution Control Assoc., annual, Louisville, Ky. (R. Bourne, APCA. Room 2, City Hall, Louisville)

21–22. Conference on Radiofrequency Spectroscopy in Solids, Bangor, Wales. (Physical Soc., 1 Lowther Gardens, Prince Consort Rd., London, S.W.7, England)

21–23. French Medical Congr., 33rd, Paris. (C. Laroche, 34 rue de Bassano, Paris 8)

24–27. American Inst. of Chemical Engineers, Lake Placid, N.Y. (E. R. Smoley, 30 School Lane, Scarsdale, N.Y.)

25–29. European Committee of Liaison for Cellulose and Paper, symp., Oxford, England. (British Paper and Board Makers' Assoc., Technical Section, St. Winifred's, Welcomes Rd., Kenley, Surrey, England)

25–30. Magnetism and Crystallography, intern. conf., Kyoto, Japan. (Science Council of Japan, Ueno Park, Tokyo)

26–30. European Congr. of Aviation Medicine, 6th, Paris, France. (CERMA, 5 bis Avenue de la Porte de Sèvres, Paris)

27–3. International Union of Theoretical and Applied Mechanics, Kiev, U.S.S.R. (Y. A. Mitropolsky, Scientific Committee, Kalinin pl. 6, Mathematical Inst., Kiev)

28–29. European Conf. of Chemical Engineers, Toulouse, France. (Soc. of Industrial Chemistry, 28 rue Saint-Dominique, Paris 7, France)

October

1–3. Council for Intern. Organizations of Medical Sciences, Paris, France. (CIOMS, 6 rue Franklin, Paris 16)

1–4. Process Engineers, annual, Vienna, Austria. (Osterreichischer Intenieur- und Architektenverein, Eschenbachgasse 9, Vienna 1)

1–5. Electrochemical Soc., Detroit, Mich. (ES, 1860 Broadway, New York 23)

1–7. International Special Committee on Radio Interference, plenary session, Philadelphia, Pa. (S. D. Hoffman, American Standards Assoc., 10 E. 40 St., New York 16)

1–8. International Congr. of Industrial Chemistry, 33rd, Bordeaux, France. (Société de Chimie Industrielle, 28 rue Saint-Dominique, Paris 7, France)

2–4. Communications Symp., 7th natl., Utica, N.Y. (R. K. Walker, 34 Bolton Rd., New Hartford, N.Y.)

2–7. Climatic Change, symp., Rome, Italy. (UNESCO, Place de Fontenoy, Paris 7, France)

2–7. International Astronautical Federation, 12th congr., Washington, D.C. (American Rocket Soc., 500 Fifth Ave., New York 36)

2–7. Inter-Regional Leprosy Conf., Istanbul, Turkey. (WHO, Regional Office for Europe and Regional Office for the Eastern Mediterranean, 8 Scherfigsvej, Copenhagen Ø, Denmark)

2–11. International Council for the Exploration of the Sea, 49th annual, Copenhagen, Denmark. (Charlottenlund Slot, Charlottenlund, Denmark)

3–5. Physics and Nondestructive Testing, symp., Argonne, Ill. (W. J. McGonnagle, Argonne Natl. Laboratory, 9700 S. Cass Ave., Argonne)

3–8. Aerosol Congr., 3rd intern., Lucerne, Switzerland. (Federation of European Aerosol Assocs., Waisenhaustrasse 2, Zurich, Switzerland)

4–10. Latin American Congr. of Electroencephalography, 5th, Mexico, D.F. (J. Hernandez Paniche, Instituto Mexicano de Seguro Social, Hospital La Raza, Mexico, D.F.)

4–10. Latin American Congr. of Neurosurgery, 9th, Mexico, D.F. (J. H. Mateos, Tonalá No. 15, Mexico 7, D.F.)

6–7. American Medical Writers' Assoc., New York, N.Y. (S. O. Waife, P.O. Box 1796, Indianapolis 6, Ind.)

6–8. Therapeutics, 7th intern. congr., Geneva, Switzerland. (P. Rentchnick, Case Postale 229, Geneva 2)

8–10. Zooplankton Production, symp., Copenhagan, Denmark. (J. H. Frazer, Marine Laboratory, P.O. Box 101, Victoria Rd., Aberdeen, Scotland)

8–11. Society of American Foresters, Minneapolis, Minn. (H. Clepper, SAF, 425 Mills Bldg., Washington 6)

8–13. American Acad. of Ophthalmology and Otolaryngology, Chicago, Ill. (W. L. Benedict, 15 Second St., SW, Rochester, Minn.)

9–11. National Electronics Conference and Exhibition, 17th annual, Chicago, Ill. (NEC, 228 N. La Salle St., Chicago, 1)

9–12. Instrument Symp. and Research Equipment Exhibit, 11th annual, Bethesda, Md. (J. B. Davis, Natl. Institutes of Health, Bethesda 14)

9–12. Water Pollution Control Federation, 34th annual, Milwaukee, Wis. (R. E. Fuhrman, 4435 Wisconsin Ave., NW, Washington 16)

9–13. American Rocket Soc., space flight meeting, New York, N.Y. (ARS, 500 Fifth Ave., New York 36)

9–13. Luminescence of Inorganic and Organic Systems, intern. conf., New York, N.Y. (Miss G. M. Spruch, New York Univ., Washington Sq., New York 3)

10–12. Nuclear Reactor Chemistry, 2nd conf., and Analytical Chemistry in Nuclear Reactor Technology, 5th conf., Gatlinburg, Tenn. (Oak Ridge Natl. Laboratory, P.O. Box X, Oak Ridge, Tenn.)

10–20. International Committee for Biological Control, Tunis. [P. Grison, Laboratoire de Biocenotique et de Lutte Biologique, La Miniere, par Versailles (S.-et.-O.), France]

11–13. Gaseous Electronics Conf., American Physical Soc., Schenectady, N.Y. (C. J. Gallagher, General Electric Research Laboratories, Schenectady, N.Y.)

11–14. Tau Beta Pi Assoc., Cincinnati, Ohio. (R. H. Nagel, Univ. of Tennessee, Knoxville)

11–14. Western Inst. on Epilepsy, 13th annual conf., San Antonio, Tex. (F. Risch, 3097 Manning Ave., Los Angeles, Calif.)

12–13. Congress of Neurological Surgeons, New York, N.Y. (E. Weiford, 4706 Broadway, Kansas City 12, Mo.)

12–29. Pacific Intern. Trade Fair, 2nd, technical meetings, Lima, Peru. (PITF, P.O. Box 4900, Lima)

14–20. International Congr. of Neurological Surgery, 2nd, Washington, D.C. (B. S. Ray, 525 E. 68 St., New York 21)

15. American College of Dentists, Philadelphia, Pa. (O. W. Brandhorst, 4236 Lindell Blvd., St. Louis, Mo.)

15–20. American Inst. of Electrical Engineers, fall general meeting, Detroit, Mich. (E. C. Day, AIEE, 33 W. 39 St., New York 18)

15–20. International Congr. of Allergolgy, 4th, New York, N.Y. (W. B. Sherman, 60 E. 58 St., New York 22)

15–21. Pan American Congr. of Endocrinology, 5th, Lima, Peru. (M. San Martin, Av. Central 325, San Isidoro, Lima)

16–17. Engineering Writing and Speech, natl. symp., East Lansing, Mich. (J. D. Chapline, Philco Corp., 3900 Welsh Rd., Willow Grove, Pa.)

16–17. Ionization of the Air, intern. conf., Philadelphia, Pa. (I. C. Kornblueh, American Inst. of Medical Climatology, 1618 Allengrove St., Philadelphia 24)

16–18. American Soc., of Safety Engineers, Chicago, Ill. (A. C. Blackman, 5 N. Wabash Ave., Chicago 2)

16–18. Entomological Soc. of Canada and Entomological Soc. of Quebec, Quebec, Canada. (L. L. Reed, ESC, Neatby Bldg., Carling Ave., Ottawa, Canada)

(See issue of 18 August for comprehensive list)

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Applications for the above position, including a curriculum vitae, the titles of publications, a recent photograph and the names of three referees, should be sent to the **DEAN OF MEDICINE, UNIVERSITY OF ALBERTA, EDMONTON, ALBERTA, CANADA.** 9/1

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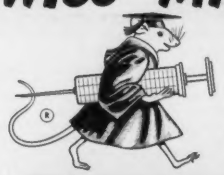

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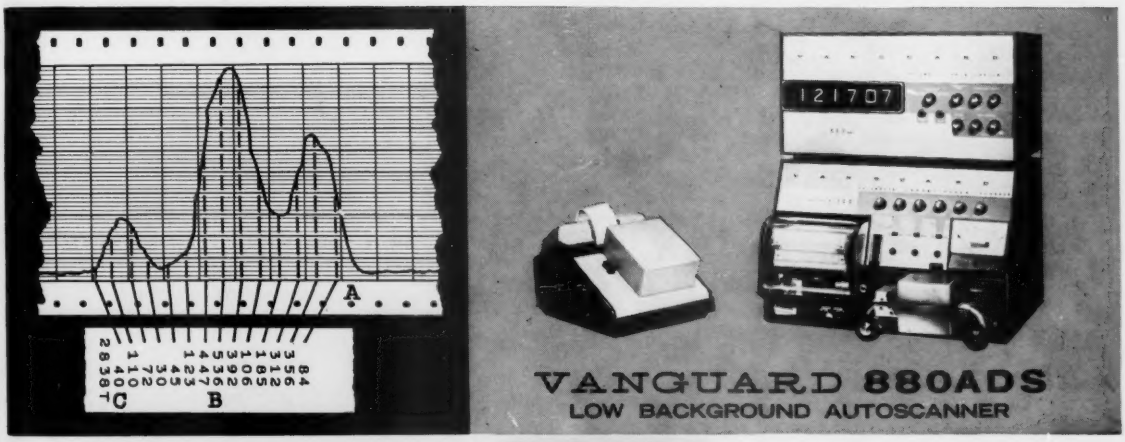

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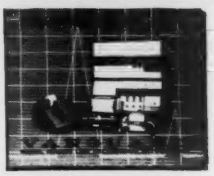

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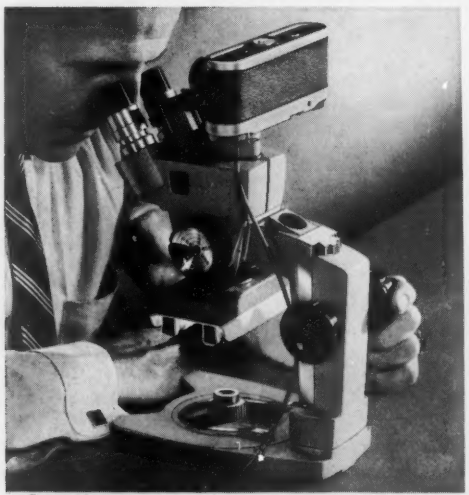

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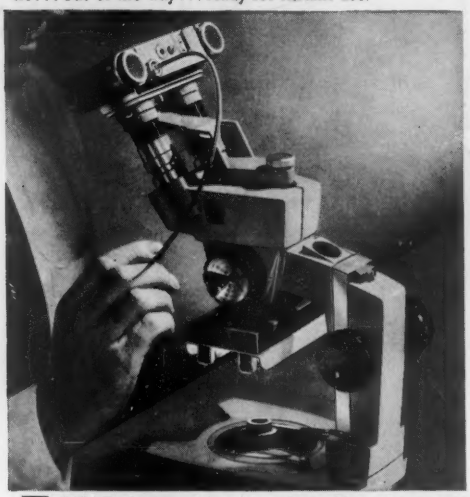

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